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Scientific Handbooks.

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The fourth volume, which is just published, contains the only popular account extant of the science of Geodesy, written by Professor J. Howard Gore, of the Columbian University.

Other volumes will follow, written, like these, by thoroughly competent writers in their own departments; and each volume will be complete in itself.

Heinemann's Scientific Handbooks

GEODESY

By J. HOWARD GORE

THE EARTH AS A FLOATING EGG.

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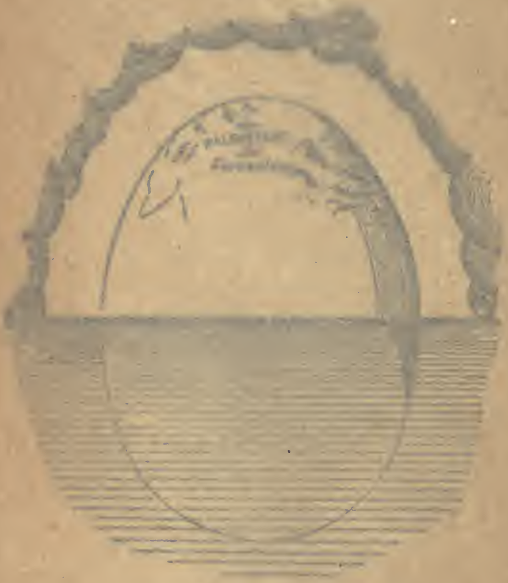

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PREFACE.

At this busy period it is hardly needful to apologize for the preparation of an historic sketch of any of the sciences. The literature of each branch is becoming so extensive that only a few can take the time to study it through, and upon them is it incumbent to give to others résumés of what they read. In a subject like Geodesy, describing work prosecuted in many lands and at divers epochs, it becomes a difficult matter for even the few to succeed in exhausting the major part of its literature. It is my good fortune to possess the original reports of these operations — a possession in which no library is a sharer, and therefore I have found it a genuine pleasure to present this sketch of Geodesy. It is simply a sketch: a critical history will be the labor of future years.

As all the data have been taken from first

hand, it has not been deemed necessary to make in every case the usual acknowledgments, but it is due my friend and pupil, Miss Cora E. Dill, that I give expression to my thanks for her painstaking reading of the proof.

J. HOWARD GORE.

COLUMBIAN UNIVERSITY.
May, 1891.

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GEODESY.

CHAPTER I.

SOME PRIMITIVE NOTIONS.

WHAT do we mean by primitive notions? Not alone such as have found lodgment in the minds of ancient peoples, but those which were first deemed sufficient to account for observed phenomena. As soon as a fact is perceived, a great advance is made; nor is progress arrested before a cause is formulated; the existence of a problem tempts a solution, the propounded question challenges an answer. Primitive ideas exist to-day as well as when the first man let fall his questioning gaze upon the unknown around him. A North American Indian at this very hour may be laying down an hypothesis to satisfy some unsolved problem that is just as crude as the one which came for that same problem to the East Indian centuries ago. Both were primitive in the intellectual development of the originators, though one far antedated the

other in time. Hence, in dealing with primitive notions regarding the shape or size of the earth, chronology cannot play an important part; the treatment as to time must be arbitrary.

To a person on a great eminence, the earth appears as a vast plain, with the horizon supporting the sky as a dome-shaped roof. This appearance has been noticed by peoples at all times, and has called into existence flat-earth theories without number. A spirit of patriotism has prompted all to make one's own country occupy the most prominent geographical position and assume the maximum importance. Complimentary and laudatory myths have been invented by each tribe to account for the formation of its own country, while averring that all other tribes occupy insignificant areas formed of the useless fragments their builders graciously donated. Nor has this spirit been restricted to unlettered tribes; it showed itself long after the art of map-making was known, rendering it possible to determine from what country a map emanated by the relative sizes of the countries thereon. Likewise, as one looks out from a suitable position, one sees the horizon like a circle whose centre is the observer, and the sky above apparently a globe with the same centre. Thus it is that the Hindoos at the equator and the Scandinavians near the pole apply each a name to their

own country which signifies the "central habitation."

The "hub" theory is not restricted to one country alone. The Greeks made Olympus the centre, the Egyptians Thebes, the Assyrians Babylon, the Hebrews Jerusalem; while the Chinese have regarded their land as the central empire since the time they sent astronomers to the four points of the compass to locate the equinoxes and the solstitial points.

One of the most primitive ideas regarding the earth represented it as an immense plain, or flat island, surrounded on all sides by an interminable ocean. This ocean in the minds of the Greeks was only a river, called Okeanos, and into this the sun made each night a plunge, to arise in the morning on the opposite side of the earth. At the extremities and borders were placed the "fortunate isles," or imaginary regions inhabited by giants, pygmies, and such mythical creatures as a vivid imagination could call into being. But when men began to have experience of the sea by navigation, the horizon always observed as circular led to the notion that the ocean was bounded, and the whole earth came to be represented as a circle, beneath which were roots reaching downward without end.

The cooling of the sun from his daily bath demanded some change in the material into

which he plunged. And as he could not go down on one side and come up on the other without making the subterranean journey, some provision had to be made for his passage. It was easy to imagine a tunnel through which he could pass, but as soon as the progressive and retrograde movements in his places of setting and rising were recognized, it was necessary that the support of the earth be honeycombed with passages. The Buddhist priests declared that the sun passed between the pillars which supported the earth. And no sooner did they find this theory acceptable than they applied it to their ends. For, said they, these columns are sustained by virtue of the sacrifices which were made to the gods, and any indifference on the part of the worshipers might cause a collapse of the earth.

The ancient Greenlanders affirmed that the earth is upheld by pillars which are so consumed by time that they crack, thus quaking the earth; and were it not for the incantations of the magicians, the earth would long since have broken down. Thus we see a myth reaching

> "From Greenland's icy mountains
> To India's coral strand."

The Hindoos held the earth to be hemispherical, and to be like a boat turned upside down

upon the heads of four elephants which stood on the back of an immense tortoise. This support, like a superficial answer, was sufficient until some curious questioner insisted upon knowing upon what the tortoise rested. The answer, upon the universal ocean, was soon proffered and gladly accepted, until the application of the further test, on what does the ocean stand? The ultimatum was then reached; the theory so boldly advanced and so ingeniously sustained needed a foundation principle; this it received in a shape that stilled further doubts and strengthened the whole superstructure: what supports the ocean? why, it goes all the way to the bottom. This form of reply did not disappear with those who first made it, nor has it found a place in earth theories alone. Almost every science has advanced its line of interrogation points until a final all-sufficient answer is given or an accepted axiom quoted.

Anaximander, a philosopher of the sixth century before Christ, represented the earth as a cylinder, the upper face only being inhabited. By some process now not known he computed its proportions, and gave as the result that its height was one third of its diameter, and that it floated freely in the centre of the celestial vault. The doctrine of "sufficient reason" prevailed then, or was invented to fit this particular case,

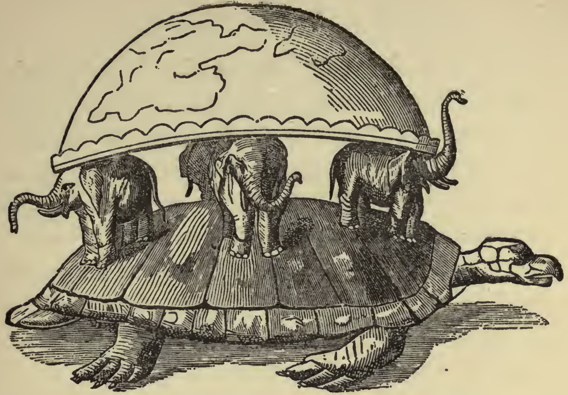
because when asked why this cylinder did not tip over, he replied that in the absence of a reason why it should tip in any one direction rather than in another, it did not tip at all, hence remaining in this state of helpless indecision. A fellow philosopher, recognizing the importance of air in the economy of nature, supported the cylindrical earth of Anaximander on compressed air, which, owing to the vague and apparently imponderable character of air, did not suggest the need of a resting-place for this air-cushion.

Aristotle relied more upon fancy than upon fact, and deduced conclusions from logic and the nature of things rather than from observation. He reasoned from what he deemed natural to what must be or is. In answer to the question, Is the earth at rest? he replied, "The earth is in repose, because we see it to be so, and because it is necessary that it should be, since repose is natural to the earth." He also affirmed that a circle is a perfect line, being uninterrupted and without ends; and therefore the stars, created by God to endure forever, must have an eternal motion, and being perfect creations they must move in the perfect line — the circle. The heavens, also, possessing this divine attribute of perpetuity, must move in a circle. Then, as a grand climax, he asserted that since in every revolving circle there is a point of absolute repose,

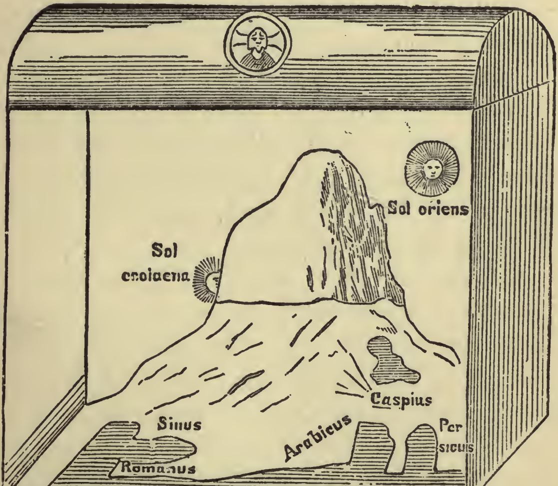
the centre, the earth, being at rest, must occupy this central point.

Strabo, the geographer, who made the first century of the present era illustrious by his maps, declared the earth to be spherical, for in all his investigations and study regarding the travels of sailors and explorers he found no mention of the end of the earth. To him, it appeared central and motionless, and in his consistent ignorance he unyieldingly affirmed that the entire habitable globe was represented on his maps, and was in shape like a cloak 8,000 miles long and 3,600 miles in width, the greater dimension being from east to west; hence our term longitude for degrees counted in that direction.

During the first centuries of this era, the chief expounders of science were the fathers of the church, who satisfied themselves by teaching those principles which had been handed down bearing the stamp of approval in the transmission, and boldly refuted any belief or innovation that threatened to call in question an accepted dictum. Thus it is that we find within this period nothing new in earth theories until 535 A. D. In this year Cosmas, surnamed Indicopleustes, after returning from a voyage to India, wrote a book entitled "Christian Topography," in which he attempted to bring order



THE HINDOO EARTH.



THE EARTH OF COSMAS.

out of the chaotic views of the fathers, and to explain the universe in harmony with observed phenomena and in accordance with Scripture.

According to Cosmas and his map of the world, the habitable earth is a plane surface; but instead of a disk, he represented it to be in the form of a parallelogram, whose long sides were twice as long as the shorter ones, with man living on the interior of one of these sides like a bird in a cage. The portion inhabited is surrounded by an ocean, which breaks in in four great gulfs: the Mediterranean and Caspian seas, and the Persian and Arabian gulfs.

Beyond the ocean in every direction there exists another continent which cannot be reached by man, though one part of it was occupied by him before the deluge. Here he placed Terrestrial Paradise. After his fall Adam was driven from Paradise, but he and his descendants remained on the coasts until the deluge carried Noah to our present earth.

On the four edges of the earth rise four perpendicular walls which inclose it, and coming together at the top, they form the heavens like a cupola. The world then is divided into two parts: the first, the abode of men, reaches from the earth to the firmament, above which are the stars impelled by angels, who either carry them on their shoulders, roll them along, or draw

them behind, each angel that propels a star taking great care to observe what the others are doing, so that the relative distances between the stars may always remain what they should,—carelessness in this important matter often resulting in collisions, as is evident from eclipses and falling stars.

The reason for selecting this shape and these dimensions is that the form given for the Tabernacle must have possessed some advantages, since it was so accurately described and so strenuously insisted upon, which advantage would obtain in the case of the world. The sun, moon, and planets all set behind a large conical hill or mountain, and according as the sun disappears and emerges at points higher or lower is the varying length of the day and night.

All this structure, he affirms, rests on nothing, but is sustained by the word of God's power; making it the base of the universe, He ordained that it should sustain itself by its own proper gravity.

Bede, known as the Venerable, who lived in the eighth century, regarded the earth as formed upon the model of an egg. Being an element, it is placed in the middle of the universe as the yolk is in the middle of the egg; around it is the water, like the white surrounding the yolk; outside that is the air, like the membrane of the

egg; and around all is the fire, which closes it in as does the shell. The earth, being thus in the centre, receives every weight upon itself; and though by its nature it is cold and dry in its different parts, it acquires, accidentally, different qualities; for the portion which is exposed to the torrid action of the air is burned by the sun, and is uninhabitable; its two extremities are too cold to be occupied, but the portion that lies in the temperate region is habitable. The ocean which surrounds it by its waves as far as the horizon divides it into two parts, the upper of which is inhabited by us, while the lower is inhabited by our antipodes; although not one of them can come to us, nor one of us go to them.

As in other theories, a support was needed. To meet this requirement Edrisi, an Arabian geographer, broached the idea that the egg-like earth floats in the great ocean as in a basin.

Although we are told that Pythagoras and Thales taught that the earth was spherical, we see that their teaching was without avail on this point for nine centuries, while the shape of the earth was the play of many foolish fancies. Bede again rounded it off and gave to it the egg shape which it retained in the minds of men for a thousand years. Many of these theories were so erroneous that they were in nowise links in a great chain which could bind all peo-

ples into unanimity of belief. But while many were beating time without marching, others were making progress along hopeful lines. To show how this has been accomplished will be the purpose of the succeeding chapters.

CHAPTER II.

PRIMITIVE DETERMINATIONS OF THE SIZE OF THE EARTH.

Now that the development of ideas had carried men far enough to accept a spherical globe, it was only natural that speculations were rife as to its size, and it was equally natural that some one should come forward with a method for determining this magnitude; and while this method might in itself be inaccurate or unsatisfactory, yet it would serve as a quickening force in the elaboration of better and still better plans.

Before attempting to measure the entire earth it was necessary that considerable success should have been met with in measuring limited portions of its surface. Just when this art was first practiced is hard to ascertain; probably when Joshua was sent to spy out the land, he had in view quantity as well as quality.

We are told by Herodotus that the credit of discovering geometry belongs to the Egyptians, who found need of its principles in the restora-

tion of those boundaries of fields which were obliterated by the overflow of the Nile. He also gives us the method of procedure when the river god consumed portions of a man's landed possessions. "If the river carried away any portion of a man's lot, he appeared before the king and related what had happened, upon which the king sent persons to examine and determine by measurement the exact extent of the loss; and thenceforth only such rent was demanded of him as was proportionate to the reduced size of his land. From this practice, I think, geometry first became known in Egypt, whence it passed into Greece."

Although numerous architectural plans of tombs of kings have been found, drawn approximately to a scale with the dimensions given in cubits, no plots of land made by the surveyors of Rameses have been discovered.

Recent discoveries made among the cuneiform inscriptions at the British Museum on terra-cotta tablets suggest that the Babylonians antedated the Egyptians in the application of geometry to land-surveying or at least to cartography. As is well-known, the country lying between the Tigris and the Euphrates was early occupied by a land-owning people. It, like the fertile fields bordering the Nile, was subject to a periodic overflow, and the consequent elimina-

tion of landmarks, except such as were erected with a view to permanency.

The real definition of the boundaries of land was effected in Babylonia by corner stones, on which were carved, not only the metes and bounds, but words which constituted the stone itself the deed of sale. One of the most noticeable of these boundary stones in the British Museum is a large stone bearing an inscription of Merodach-baladan [B. C. 1200]. It records a gift by the king of a plot of land to a person named Merodachsum-Izakir, as a reward for political services. It gives no dimensions, but carefully describes all adjoining properties, and is attested by many witnesses.

There is also a plan drawn on a tablet in dark terra-cotta, about 6 inches by $3\frac{1}{4}$ inches, and represents a plot of land about $8\frac{1}{2}$ acres. The inscription at the top informs us that it is the plot of a field in the high road on the banks of the river or canal. The estate is divided into three pairs of parallelograms, to which are added two more similar shaped plots, and an irregular trapezoidal piece. The dimensions are all given in cubits or fractions of cubits, most carefully figured on the drawing. The northern boundary is the highway. The western side adjoins the lands of Ipruja and Buruga, the sons of Taria, who also owns the land on the south. The eastern adjoins the lands of Nabusar.

The names of the seven witnesses, who attest the deed by affixing their nail-marks, and the scribes, who append their seals, testify to the legal character of the document now carefully preserved centuries after its makers passed into oblivion.

If thus early in the world's intellectual life men knew so well how to measure small areas, we may readily accept the belief that the greater problem was soon the subject of attack.

It is said that the Chaldeans were the first to make an estimate of the earth's circumference, but one would search in vain the Sanskrit writings — the scientific language of Chaldea — for an account of this estimate; it is found only amongst the Arabians and Greeks.

In an Arabian manuscript recently discovered, it is said that according to the Chaldeans 4,000 steps of a camel make a mile, and $33\frac{1}{2}$ miles correspond to a half-degree, from which the entire circumference of the earth must contain 24,000 miles. There is unfortunately no perfect consistency in the steps of a camel, nor can its length be approximated with an accuracy demanded by a modern critic. It is most likely that this value was taken from the Almagest of Ptolemy, in which the circumference is given as 180,000 stadia, with the remark that $7\frac{1}{2}$ stadia are equivalent to the Chaldean mile. Still the

glory is not so easily wrested from these shepherd-astronomers. The Grecian Achilles states that amongst these people there was current the notion that a man who is a good walker will, if he does not stop on the way, complete the circumference of the earth in a solar year, by going always towards the east. Here again the uncertainty as to what was regarded as a good rate for a walker, and the doubt whether the sun was followed in the walk, make for the Chaldeans but little honor except for ingenuity and priority.

The Greeks are liable to fare better in any claim which they set up. Having historians and a language of their own, their theories as well as exploits were duly recorded, preserved, and transmitted. Yet there is a mist of uncertainty lingering around the earliest statements about the way in which the Greeks tried to solve the problem. Aristotle in a fragmentary way, with the key-word wholly lacking, refers to a measurement or an approximation of the earth's circumference. It is most likely the latter, since he has stated in another place that then, as well as formerly, distances were estimated only by days' journeys or by the journey of a day and night.

About 100 years later Archimedes informed us that other geometers had attempted to show

that the earth was about 30 myriad stadia in circumference, but that he, more generous than they, multiplied their value by ten, and affirmed that 300 myriad stadia will represent that magnitude. It is evident that this number was given principally because of its roundness. If these results were the outcome of observation they deserve but little more credence than if they were simply approximation, since the instruments of that time were quite unreliable, as Archimedes himself admits. Some persons, anxious to give antiquity to earth measures and to ascribe as much glory as possible to the Greeks, find in Cleomedes a comforting clause. It is this: "The head of the Dragon crosses the zenith at Lysimachia while the head of the Crab is on the zenith at Syene. The arc between the Dragon and the Crab is one fifteenth of the circumference, as is found by the gnomon, and the distance between these two places is 20,000 stadia." The temptation to multiply the 20,000 by 15 is very great, and to say "there is the 30 myriad," but unfortunately Cleomedes had nothing in view except to show that the earth is not flat. There is likewise an astronomic obstacle in the way. The declination of the middle point of the head of the Dragon is about 52° 30′, from which the obliquity of the ecliptic deducts 23° 28′; then Syene was always regarded

as under the tropic of Capricorn. This would give 29° for the terrestrial arc joining those places, and not 24° or one fifteenth of a great circle. With this palpable error before us, one can only effect reconciliation by supposing different stadia were employed.

In order to fully understand what follows, a slight digression must be permitted wherein the principal mathematical instruments will be described. The first of these is the gnomon, or shadow-measurer. This instrument, which was unquestionably known to the Chaldeans and the ancient Egyptians, is said to have been first used by Anaximander or his pupil Anaximenes. It consisted of a pin standing perpendicularly upon a horizontal plane on which it cast its shadow. A simple trigonometric computation will give the altitude of the sun from the known height of the pin and the length of the noonday shadow. It would be difficult to secure anything like accurate results, where a slight error in measuring the length of this shadow is inevitable, owing to the uncertainty as to where the shadow ends. Perhaps in Egypt the obelisks were used as gnomons, but there the exact height also would be a doubtful quantity. This instrument received a most important improvement at the hands of Aristarchus, by which the angular altitude was given directly without

any computation. He substituted for the plane a hemispherical bowl, and placed in its lowest interior point a peg in length equal to the radius and perpendicular to the plane to which the bowl was attached. Concentric equidistant circles, drawn about this peg on the inner surface of the bowl, formed the scale by which the sun's altitude was measured from its shadow. This instrument, called the *scaph* because of its shape, involved the same source of error in reading that obtained in the gnomon, but eliminated the errors of computation. This invention, however, is of great importance, since it was the forerunner of all angle-measuring devices in which a graduated circle is employed.

The gnomon, which at once assumed the form of the scaph, but retained its old name, was used to determine the latitude of a place. On the equinoctial day, the noon sun casts no shadow at the equator, hence the angular length of the sun's shadow at any other point on this day when he is on the zenith will give the angular distance of that point from the equator or its geographical latitude. It will be seen at once that in this method the shadow cast by the middle of the sun cannot be ascertained, and that in reality the shadow cast by the northern limb was taken, thus introducing an error equal at least to the semi-diameter of the sun. From

recent astronomic determinations at many points, the errors in latitude as given by the users of the gnomon are from fourteen to fifteen minutes. With such an instrumental outfit, it is remarkable that even a theoretical method for determining the size of the earth should suggest itself. Yet not only was the theory advanced, but the plan was put into practice and we have the results.

Eratosthenes deserves the credit for devising a method of earth or degree-measuring which in principle is still used for that purpose. He was born at Cyrene in 276 B. C., studied philosophy with Ariston, and poetry, the handmaid of the sciences, with Callimachus. About 240 B. C. his scholarship attracted the attention of Ptolemy Euergetes, who, anxious that Alexandria might be regarded as the scientific centre of the world, added a large store of books and instruments to the Alexandrine library and gave the position of librarian to Eratosthenes. Here he became actively interested in the exact sciences and did much to advance their study in the School, or Academy of Sciences, made illustrious by the names of Aristarchus, Hipparchus, and Ptolemy. By them mathematics was cultivated, and astronomy lost its astrologic features and became based upon actual observation. At the age of eighty he lost his eyesight, and rather than live in darkness surrounded by

so much to see and observe, he took his life by depriving it of its sustenance. He was the author of a systematic Universal Geography and the compiler of numerous treatises on mathematical and astronomical topics, but of all only fragments have been handed down to us. Even regarding the work here to be discussed, we have only the record preserved for us by Cleomedes.

According to Eratosthenes, Syene and Alexandria lay under the same meridian, and since the meridian in the heavens belongs to the greatest circle, the terrestrial arc lying under it upon the earth must be the greatest. Hence the circumference of the earth will be the length of the great circle passing through Syene and Alexandria. And, since Syene is under the tropic of Cancer, the sun when on the zenith at the summer solstice will cast no shadow of an erect object. But at Alexandria, at the same time, the sun will cast a shadow in the gnomon, this place being farther north. If now one imagines the sun standing still, with respect to the earth's surface, and the gnomon to be transferred towards Syene, it is easy to see that the shadow in the gnomon will gradually shorten until it becomes nothing at Syene. In this transfer the shadow in the instrument passes over an arc of the same amplitude or angular distance as that which

joins the two points where the observations were made. This angle in the scaph was a fiftieth part of a great circle, or 7° 12′, hence the amplitude of the arc joining Alexandria and Syene was 7° 12′. The linear distance between these places was regarded as 5,000 stadia, therefore the circumference of the earth must be 250,000 stadia.

From even a superficial glance it must be seen that the parallelism of the sun's rays was at that time accepted as a fact, and that at least three geometric theorems were established: that the earth is globular, that equal arcs subtend equal angles, and that lines which meet at a great distance are parallel.

Although Cleomedes is the only author who describes the method of Eratosthenes, the results of his determination have been given by several Greek and Roman writers, all of whom, however, give 252,000 stadia. It is quite likely that Eratosthenes added the 2,000 stadia so as to have one degree expressed in a round number, 700, instead of the awkward $694\frac{4}{9}$. This slight doctoring of results, he might regard as permissible on account of the uncertainties in his angular and linear factors.

Eratosthenes in this critical age can expect nothing more than credit for suggesting to his successors a principle which in its elaboration

and refinement serves as the foundation of degree-measuring. Unfortunately for any claim to greater glory, he omitted some essential precautions and adopted unreliable data. As already suggested, the sun's semi-diameter was neglected; then the termini of his base were 3° apart in longitude, and not on the same meridian as he inferred, and the position of Syene was not on the tropic of Cancer. Again, from recent determinations, the latitude of Alexandria is such as to increase the amplitude of Eratosthenes' arc by 15′ more. There are also grave doubts as to how he obtained the distance from one end of his arc to the other. In his Geography he gives 5,300 stadia as the length of the Nile between these points, so he must have thrown away the extra 300 stadia for the crooks and turns in the river.

Luckily for this pioneer geodesist, we can only point out the *sources* of error. Just what their *extent* is we shall never know, owing to the impossibility of ascertaining what unit of length he employed.

Cleomedes added an extension to the plan of Eratosthenes, in suggesting that at any time one might erect at Alexandria and Syene gnomons and read in each the shadow cast on the same day by the noon sun, the difference in the lengths of these shadows giving the amplitude. This is a

perfect prototype of the present method of finding the difference in latitude of two places.

The next person to attack this problem was Posidonius, born at Apanea, in Syria about 135 B. C. He was a Stoic philosopher of great renown, and in his extensive travels he met Cicero and Pompey, becoming their firm friend, some say their instructor. At all events, it is narrated that when Pompey visited him the lictors were required to lower their fasces in acknowledgment of his greatness. Posidonius was a careful and thoughtful observer of natural phenomena. He saw that the moon when near the horizon apparently increased in size; this he accounted for by saying the vapors of the atmosphere broke the rays of light, and, turning each from its direct line, amplified the image. The rise and fall of tides did not escape his notice, for he left behind the statement that the tides are highest at new and full moon, and lowest at the quarters.

It is not at all surprising that such a keen observer and broad generalizer should make a valuable contribution to human knowledge regarding the *size* of the earth. The matter of shape did not apparently trouble him; he accepted without questioning the globular theory.

Here again we have only the report of Cleomedes. He says: "According to Posidonius,

Rhodes and Alexandria are upon the same meridian, and the distance between these two places appears to be 5,000 stadia, and that value may be accepted as correct." While on his journeyings, he had noticed that Canopus, the brightest star in the southern heavens, was visible from some places and not from others, and that the points of visibility were always to the southward. At Rhodes he says he saw this star on the horizon, but only an instant, for the revolution of the heavens soon carried it under. He had divided the zodiac into twelve signs of equal arc lengths, and each of these parts he quartered, making 48 parts into which the zodiac was subdivided. Then considering every great circle to be similarly divided, he applied himself to the task of seeing how high on this scale Canopus was at Alexandria when culminating. This he found to be equal to one of his divisions or one forty-eighth of a circumference. Hence, the distance between Rhodes and Alexandria was an arc of 7° 30′, and the circumference of the earth is 48 times the 5,000 stadia between these two cities, or 240,000 stadia.

The mathematics of this operation is good, and is considerably in advance of the principle underlying the method of Eratosthenes, but the application contains such large errors that one must doubt whether Posidonius actually made

this determination or simply took his angular and linear data hypothetically, saying that then the circumference will be the value named above. There is an error of more than two degrees in the amplitude of this arc; the difference in longitude of the two stations is about two degrees; and the terrestrial distance is only a sailor's estimate, — two egregious errors and one doubtful approximation, — three sources of uncertainty, one of which would be sufficient to vitiate the result.

It is unfortunate that no mention has been made of the instrument used by Posidonius in measuring the altitude of Canopus. At that time there were known three instruments for measuring angles: triquetrum, — three rulers, one of which was held vertical, another was directed towards the object whose distance from the zenith was to be determined, while the third measured the distance between fixed points on the other two, the angle being taken from a table or computed from the known lengths of the three sides; astrolabe, an invention of Hipparchus; and the diopter, a ring, probably graduated, provided with a ruler, which one could move about the centre of the ring.

If we knew which of these instruments he used, we would know how much confidence to place in other observations made with it.

There is another result ascribed to Posidonius. This is found in Simplicius, who says, "The astronomers seek by means of the diopter two stars that are distant from one another just one degree of celestial arc. They then determine the positions of two places on the earth through whose zeniths these stars pass and then measure their distance apart. In this manner they found that a degree contained 500 stadia, from which they concluded that there was in the entire great circle of the earth 18 myriad stadia." This value had the indorsement of many of the ancients but is in itself of no worth.

Here again, however, our ignorance of the unit of measure used in both of these determinations precludes an exact statement of the errors in the results.

Just here we must take a long step chronologically but not geographically. The exact sciences ceased to be cultivated in Greece and Egypt. They slumbered for centuries and awakened in Arabia. When the Arabians adopted the Mohammedan religion they became ambitious for mastery, not alone in the field of battle, but in the arts and sciences. They embraced, extended, and utilized the knowledge they found during their occupation of Egypt. They preserved trigonometry from oblivion and handed it down to us in its present shape, while

to them we are indebted for the beginning and early development of practical astronomy. The Arabians reached their zenith under Caliph Almamon. He was not only a scholar, but a patron of the sciences, and assembled about him the most learned men of that period, among whom were Acaresimi, Alfraganus, and Albategni. It is in their treatises that one finds the first mention of *sines*, also tables of sines with an explanation of their use.

This wise caliph was the next to make a contribution to the world's knowledge of the size of the earth. In 819 he imposed upon his astronomers the task of measuring a meridional arc on the plain of Singar by the Arabian Sea. They divided themselves into two parties, and starting from a given point, one party went north, measuring with wooden rods as they went, the other due south, likewise measuring as they went. Each party continued, the former until they reached a point where the altitude of the pole was just one degree higher than it was at the starting point, while the other did not stop until they found a place where the altitude had decreased by one degree. Thus we see both groups had gone just a degree. The northward party had measured 56 miles, the southward, $56\frac{2}{3}$ miles.

It is said that they repeated their measure-

ments and obtained identically the same results. However, others affirm that, appreciating the impracticability of the successful discharge of their duties, they adopted the value given by Ptolemy, which is perhaps about the mean of the two just given.

The method here described possessed geodetic features far in advance of those employed by the Greek mathematicians, but we have no information regarding the way in which the altitude of the pole was determined. From a lack of exact data as to the length of their unit, we are unable to form any opinion as to the accuracy of their result.

For seven hundred years speculation slumbered, and investigation was ignored. A cloud of ignorance hung over the world like a pall, and the attainments of the earlier inhabitants did not even remain as a shadowy dream. The discovery of mathematical writings left in Spanish cloisters by their Moorish invaders awakened some interest in the study of the exact sciences, and from the oblivion of the dark ages arose, Phœnix-like, the genius of that grander culture then begun, now unfinished.

In the opinions of men the earth was a plane, and again there was that painful, tedious uplifting of the benighted people to the realms of truth. Fortunately the demonstrations were

not left to the theorist alone; the science of navigation had by this time enabled men to venture beyond their littoral thoroughfares, and Magellan, circumnavigating the world in his three-years' voyage, placed beyond the confines of hypothesis the globular form of the earth.

Now that the general shape was accepted, the determination of its size and figure became a matter of interest. Strange to say the method which suggested itself to Fernel in the early part of the sixteenth century was the same that Eratosthenes employed 1,700 years before.

Fernel was court physician to Henry II. of France. During his early life he spent much time in the cultivation of the mathematical sciences, but later he abandoned them to devote his time to his profession. He died in 1558, at the age of seventy-two.

Just when Fernel executed his terrestrial measurement is not known. There is no mention made of it in the sketch of his life, written by his friend Plantius, so that we must rely upon the short account given in "Cosmotheoria," by Fernel, published in Paris 1528. He, according to this report, determined with Ptolemy's rods (the triquetrum) the sun's meridian altitude somewhere in Paris on the 25th of August (year not mentioned), then he went northward, presumably towards Amiens. On

the 28th he again observed the sun's altitude, and finding that he was not yet a degree north of Paris, he continued his journey on the following day, until he reached a place which he decided was just one degree north of his starting-point. His observations were made to minutes, and even supposing that he determined the altitude to the very second, the error arising from the neglect of the sun's motion in declination amounts to 2′, which of itself would damage the result to the extent of *one thirtieth* of *his* length of the degree. If he went as far as Amiens, as many think, the amplitude of his arc would be subject to an additional error of 3′.

After satisfying himself that he was one degree north of Paris, he mounted a wagon, and counting the revolutions of a wheel as he proceeded, he started on the return journey to Paris. Just how he made allowance for turns in the road or change in inclination is not stated. He simply says the distance between these points is 17,024 revolutions or 68 Italian miles and 96 paces. This, however, he changed to 68 miles $95\frac{1}{4}$ paces to avoid fractions in the resulting value for the circumference of the earth. After giving this result he compares it with that of the Arabians, and from the close agreement he concludes that both are right, but many persons now think that he took his value from the

Arabians and worked backwards to the number of revolutions of the wheel in one degree.

Here, again, criticism is forestalled by our inability to state with certainty what mile he employed, or its exact length at the time of its use. Much valuable time has been wasted in useless and unprofitable discussion on this point, for at best we can only say that if the length which he gave for a degree were absolutely correct it would be by the merest chance, as nothing in his method or procedure could insure accuracy. But no error, however great, can increase our indifference to this crude attempt at a geodetic determination, a determination, which, historically, closes this period.

CHAPTER III.

THE BEGINNINGS OF ACCURATE DETERMINATIONS.

THE name which will always be associated with the early history of Geodesy is that of Willebrord Snell (or Snellius), a Netherland geometer. He has rendered the first years of the seventeenth century luminous by the brilliancy of his mathematical conceptions and the boldness with which he followed up his hypotheses. To him belongs the credit of having first promulgated the theory of the refraction of light and the discovery of the three-point problem, usually ascribed to Pothenot, while he enriched the literature of astronomy by the publication of an important paper on comets.

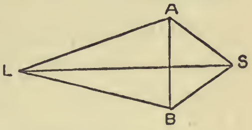
The year 1615 carries with itself the glory of having witnessed the inception of that method which will always be used in geodetic determinations — that is, the system of triangulating from a known base. In this way reducing the probability of error in its greatest source — the measurement of a long line.

As this general plan has been followed by all

who have subsequently made attempts to determine the length of a terrestrial arc, the method of Snell deserves a complete description, especially as it will elucidate much that will follow.

It is said that Ptolemy pointed out the fact that in order to determine the length of an arc it was not necessary to measure along a meridian. Still no one was willing to accept this belief until Snell demonstrated it to be true. He went still farther, saying that since it is not essential to measure on a meridian, it is not absolutely necessary that the terminal points should be connected by a straight line, but the line may be broken. This perhaps suggested at once that at least some of the lines might have their lengths computed, thereby saving the trouble of measuring them. It was of course known at that time that in a triangle, if one side and the angles be given the remaining sides can be found. The known side might be short, while the computed sides could be relatively much longer. From this it was but a step to realize that a side which has been computed in one triangle may become the known side in an adjoining triangle and aid in determining the remaining sides of the latter. Thus triangle could be joined to triangle link by link — forming what is now called a chain or net, — with only one side, called the *base*, determined by direct measurement.

His operations stretched over the arc between Alcmaar and Bergen op Zoom, and was computed in the following manner. He measured with a chain a base that was perpendicular to the line joining Leyden and Soerterwoude, A B in the appended figure; this was 326.4 Rheinish rods. With this line known and all the angles of the triangles L A B, A B S, and L A S he computed L A as a side of the

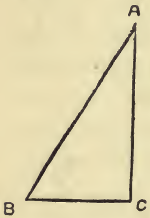


first named triangle and A S a side of the second; then with L A, A S, and the angle L A S known, the side L S was computed, giving this as a new base. Upon this he formed a new triangle, and continued so doing until he had a chain of 33 triangles reaching a church spire which he had previously selected for the terminal point.

The angles were measured with a graduated semicircle of $3\frac{1}{2}$ feet diameter. This size was necessary as it was not until sixteen years later that the vernier was introduced to aid in reading angles. This semicircle was likewise unprovided with telescopes, so that the observer had to rely on the unaided eye in sighting to distant objects. Snell realized that this crude method was liable to introduce uncertainties, so he sought to reduce them to a minimum by measuring all the angles of each triangle and

computing each side through different routes. As these variously obtained values were in all cases quite harmonious, he naturally congratulated himself upon the attainment of correct results.

Now, knowing the length of each side, the summation of a set of contiguous lines gave the length of the *broken* line joining his terminal points. However, these lines not having the same direction it was necessary to calculate what the length of each line would be if it had the direction of the line joining the ends of his arc, that is he found the *projection* of each line upon this direction; then the sum of these projections gave the distance required, which he found to be 34,710.6 rods. With this value for A B it was necessary to find A C, the meridian distance between A, Alcmaar, and C the parallel of B, Bergen op Zoom. Here again the question was simply to find the projection of A B on a north and south line. For this he needed to know in addition to A B, the angle B A C, or the *azimuth* of the oblique arc A B. For the purpose of determining this azimuth, Snell carefully fixed from astronomic observations the meridian of Leyden, and then measured at Leyden the angle which one of the



sides in his chain made with this meridian. By combining this azimuth, or angle of position as he called it, with certain angles in the chain, he obtained the azimuth of A B or the angle B A C. The result of this computation gave 33,930 rods for A C.

With the length of the arc known, nothing remained but to know its amplitude, or the difference in the latitude of Alcmaar and Bergen op Zoom. The latitudes were determined from observations made with a quadrant of $5\frac{1}{2}$ feet diameter, likewise without a telescope. The results of his observations gave : —

Lat. Alcmaar	52°	40′	30″
Lat. Bergen	51	29	00
Amplitude of the arc	1	11	30

So we have for a degree $\frac{60}{71.5}$.33930 or 28,473 rods. By taking the arc from Leyden to Alcmaar he found for a degree 28,510 rods : from these two discordant results he arbitrarily assumes 28,500 rods for a degree.

As most of the measures of this period are given in toises (a toise being approximately 6.4 feet), the length of a degree as given by Snell is 55,072 toises, counting the rod at 1.9324 toises.

Since the error in this length of a degree is practically 2,000 toises, while the method is, in general terms, correct, it is well to look into the

sources of this discrepancy. The first which attracts our attention is the fact that he regarded the surface of the earth as a plane, and computed all of his triangles by plane instead of spherical trigonometry, without making any correction for *spherical excess*, — the amount by which the three angles of a spherical triangle exceed 180°. This negligence, however, would introduce no serious error, as none of the triangles were large, and the spherical excess is only about 1″ for each 75 square miles. But in the last triangle B A C when computing the length of the *arc* A C, Snell regarded it as a *chord*, thereby making an error of one toise.

Had his vertices or stations been at different elevations it would be necessary to reduce the angle which he observed between two stations of different elevations to its equivalent in the plane of either, because his instrument was not level, but so canted as to be in the oblique plane of the two stations — so that the angle read was oblique and not horizontal. The modern method of observing with the circle level provided with a telescope having a vertical motion enables one to ascertain at once the horizontal angle between points of different elevations. Then we project all vertices upon the surface of an ellipsoid which differs the least possible from the mathematical shape of the earth and bind these

points with lines upon this surface. After deducting equally from each angle the computed spherical excess, the computations are carried forward in accordance with the established principles of plane trigonometry.

Thus far the sources of error pointed out in this pioneer work are insignificant. Those that remain are: base-measuring, angle-reading and astronomic determinations.

Snell himself suspected the existence of the two first named and went so far as to remeasure his base, which he did with wooden rods on the ice which was formed on the overflowing waters around Leyden in 1622. A triple measurement of this base satisfied him that his linear determination was good. He then repeated the measurement of the angles and found some discrepancies which caused him to say in his note-book that his last readings were the only reliable ones. By this time his advanced age prevented him from recomputing all the triangles — a labor at that time very onerous, owing to the non-existence of logarithmic tables.

About one hundred years later Musschenbroek, a descendant of Snell, revised the entire work, using a quadrant with a telescope attached. With this instrument he detected a number of errors in the angles of his predecessor, which with a new determination of the latitudes of the

terminal points gave for a degree 57,033 toises, — a value very nearly correct.

Many have been industrious in pointing out the reasons why Snell failed in obtaining a more nearly accurate result; the reasons can be easily named: the shortness of his base, the multitude of his triangles, the smallness of some of his angles, the large arbitrary corrections — sometimes as much as 3′, and his manner of observing latitudes. But it is not so easy to estimate the benefits derivable from the promulgation of this method of degree-measurement — a method so correct in its general features as to cause no delays in its elaboration, — nor to state how far the march of progress was advanced by this apparently trivial work. He deserves great honor, and all his followers must acknowledge their indebtedness to him, and not reverse the process as did Cassini de Thury, who patronizingly said of Snell, "He used the same method that the French employ, that of triangles founded on measured bases."

Richard Norwood made a backward step in his method of contributing to our knowledge of the figure and size of the earth, in that he followed the method of Fernel instead of the more exact procedure of Snell. In his "Seaman's Practice," published in 1637, he gives the following description of his measurement of a de-

gree: "Upon the 11th of June, 1635, I made an observation near the middle of the city of York, of the meridian altitude of the sun, by an arc of a sextant of more than 5 foot semidiameter, and found the apparent altitude of the sun that day at noon to be 59° 33′.

"I had also formerly, upon the 11th of June, 1633, observed in the city of London, near the Tower, the apparent meridian altitude of the sun, and found the same to be 62° 1′.

"And seeing the sun's declination upon the 11th day of June, 1635, and upon the 11th day of June, 1633, was one and the same, without any sensible difference; and because these altitudes differ but little, we shall not need to make any alteration or allowance, in respect of declination, refraction, or parallax: wherefore subtracting the lesser apparent altitude, namely, 59° 33′ from the greater, 62° 1′, there remains 2° 28′, which is the difference of latitude of these two cities, London and York."

Norwood was very explicit in describing the instrument which he used in York, but nothing regarding the kind employed in London. Then again he appears to attach great importance to the fact that the observations were made at both places on the same day. However, he deserves commendation for perceiving that refraction and parallax needed no attention when the altitudes

were so nearly the same. The linear measure was not executed with the same care, as can be seen from his own statement: "I measured (for the most part) the way from thence (York) to London; and where I measured not, I paced (wherein through custom I usually come very near the truth), observing all the way as I came with a circumferentor all the principal angles of position, or windings of the way (with convenient allowance for other lesser windings, ascents and descents), so that I may affirm the experiment to be near the truth."

This value which he regarded as true was 905,751 feet, or 367,196 feet for a degree. This equivalent in toises will be 57,300, 57,374, or 57,442, according to the different values which have been assigned as the ratio of the foot to the toise.

The absolute error committed by Norwood is about 350 toises, a result superior to what one would expect when the rude means employed are considered.

Forty years later, when Picard's results were announced, their close agreement to Norwood's prompted the secretary of the Royal Society to call to mind in the Philosophical Transactions the length of a degree as obtained by Norwood and that given by Picard, saying: "The quantity of a degree in English measures, approach-

ing very near to that, which hath been lately observed in France, he thought it would much conduce to mutual confirmation, in a summary narrative to take public notice here of the method used by the said English mathematician."

Regarding the geodetic operation which follows next in chronological order there is much doubt and confusion, nor is the exact name of the promoter known. The only record of this work is found in a report published by Picard of a journey which he made to Uranienburg in 1671. He says that he had often heard of a degree-measurement which had been made in Holland by Wilhelm Bleau, and as he had himself just concluded an undertaking of the same character, he was anxious to compare notes. He expresses the great delight which he and the "good old gentleman" experienced when they perceived that their values differed by only 60 rods. He adds that in his doubt whether Bleau's manuscript will ever be published he wishes to put on record a statement of this wonderful result, and to affirm that Snell did not accomplish a work of such importance as this.

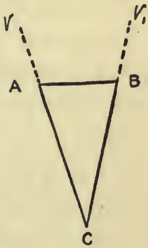
There are several reasons for believing that the narrator was anxious to find a confirmation of his own work, and was not so particular regarding accuracy in details. It is known that

Wilhelm Bleau was a most industrious geographer, celebrated for his excellent maps and his intimacy with Tycho Brahe. It is also well established that he measured the length of the shore-line from Maas to Texel and made careful astronomic observations at both points, using for the last named purpose a sector of great diameter. But no reliable information has reached us as to the length or amplitude of this arc or the length of a degree. Unfortunately for the confidence to be placed in Picard, Bleau died in 1638, thirty-three years before the visit of the former to the dismantled observatory of Tycho. Picard may have seen some of Bleau's sons and found amongst them a "good old gentleman" who was delighted at the agreement of Picard's results with that of his father. Even this favorable view is somewhat imperiled by the fact that these sons were engaged in maritime pursuits. The manuscript was never published. It most likely was destroyed by fire when the ancestral home was burned in 1672.

The Jesuits Riccioli and Grimaldi sought in 1645 to give to the world a more accurate value for the length of a terrestrial degree and the circumference of the earth. Although the former acknowledges that he had read Snell's report, he did not adopt Snell's method, but followed one of his own which is so inferior to

its predecessor that one fails to see wherein Riccioli was benefited by his reading. He decided upon two points which were to be the termini of his arc: one was the country place belonging to his order upon Mount Serra-Paderno, the other was the tower of the cathedral of Modena (Ghirlandina). Between these points the distance was determined by trigonometric operations extending through four triangles and resting upon a base 1,088.4 paces of Bologna.

Supposing the linear measures to have been exact, the question of amplitude was yet to be settled. This was avoided in the following manner: Let A and B be the terminal points, C the centre of the earth. At A they measured the angle which the vertical V makes with the line A B; the supplement of this angle is the angle B A C. By a similar operation at B, the angle A B C was found; then having the two angles, C A B, and C B A, and the side A B as determined from the triangulation, A C could be at once computed, giving the radius of the earth. Having the radius, the circumference is easily found, and taking one three hundred and sixtieth of this it gave the length of a degree.



This general plan predicated a spherical earth

and the equality of the *chord* and *arc* A B. Besides it demanded an accurately determined length for A B; for being short in comparison with A C, any error in the former would be greatly magnified in the latter. Then again it required an exact knowledge of the position of the vertical. This requisition implied absolute certainty as to the latitudes of the two points, the declination of the star which passed through the vertical of the terminal stations, and its height when crossing the vertical. As these conditions could not at that time be filled, the inaccuracy on the astronomic side must be great. The star observed at Ghirlandina was only 20° 17′ above the horizon when the vertical angle was determined, from which it is apparent that the disregarded correction for refraction would cause a serious error.

From this small arc of about one fourth of a degree Riccioli deduced in the manner just described 64,363 Bologna paces for a degree. The exact equivalent of this is not known now, but it is supposed to be about 62,650 toises.

Cassini tried to ascertain the ratio of the toise to the unit used by Riccioli by determining the distance between two of his stations in terms of the toise, and from that to get the length of the Roman mile. Subsequent reductions of Riccioli's observations reveal gross errors in his re-

sults, so that the ratio established by Cassini cannot be relied on.

While Riccioli neither by precept or example, method or result, made a contribution to the stock of human knowledge, yet some interest is attached to his name because of the use to which Mouton put his results.

Mouton, likewise a Jesuit, while organist and director of the choir in a collegiate institute in Lyons, devoted much time to the study of astronomy. He was impressed with the uncertainties and variability in the units of measure, and proposed a scheme of universal measure, "unalterable as the globe which we inhabit," and derivable from that globe. It was not only the prototype of the meter of the French Assembly in these two respects, it possessed the decimal feature also.

In 1660 he proposed that *one minute* of a terrestrial arc be called a *milliar*, one thousandth of which was to be a *virga*, the unit. Then by the scale of tens he had, *virga*, *decuria*, *centuria*, and *milliar;* and the virga, subdivided by the same scale into *decima*, *centesima*, and *millesima*.

It seems well-nigh impossible to ascribe too much glory to this neglected and forgotten astronomer. A glance at his scheme shows its superiority over the metric system. Here we

have a unit proposed which is an aliquot part of a minute: hence it is a divisor of a degree, quadrant and circle, and not a divisor of the quadrant only. The names are etymologically superior, and the use of terms from one language avoids that confusion to which the metric system is subjected in the employment of words from two languages.

In illustrating his scheme he employed the results given by Riccioli, saying: "I have such confidence in these observations that I would regard my own, if I had any, as inferior to them." He takes 321,815 paces for one degree, or 5,363.08 feet, and gives the following equivalents: —

Milliar . . .	5363.083 feet.
Centuria . . .	536.308 feet.
Decuria . . .	53.631 feet.
Virga	5.363 feet.
Decima	0.536 feet.
Centesima . . .	0.054 feet.
Millesima	0.005 feet.

And although Huygens did not publish his memoir on the isochronal character of the pendulum until 1691, and had up to this time made but one clock of any great accuracy, yet Mouton gave a series of experiments to show how the length of the seconds pendulum could be made to preserve this unit of length, and how others

could obtain the ratio of the length of any unit to his virga by ascertaining the length of the seconds pendulum in terms of the other unit. Surely he was in advance of his time.

From this it will be seen that Riccioli's unreliable results furnished important data, as did those of Picard only a few years later.

The Academy of Sciences of Paris, founded in 1666, almost from its inception seemed to regard the problem of the earth's size as its peculiar property, and we owe much of our present knowledge of this problem to the discussions called forth in the Academy's gallant defense of the results obtained by its members. In this connection it may be well to call attention to an early action of this body. They decided that if the earth revolves, a body at the equator — the point where centrifugal force would be at its greatest — would fall with less force than at the poles. The opportunity to test this matter did not present itself until 1672, when Richer, going to the Island of Cayenne, about five degrees from the equator, proposed to observe the going of a clock at this point compared with its rate in Paris. The clock, regulated by a pendulum, would go slow or fast according as the swing of the pendulum is slow or rapid, and as this depends on the force which draws the pendulum-ball from its highest to its

lowest position, wherever this force is weak the clock will go slow.

Richer found that his clock, which kept good time at Paris, lost *two minutes* and *twenty-eight* seconds a day at Cayenne, and that it was necessary to shorten the pendulum one line and a quarter — results which were verified on his return to Paris in 1673.

When Louis XIV. expressed the desire that his reign be rendered still more illustrious by having it witness operations looking towards the determination of the size of the earth, and asked the Academy, his scientific council, to nominate a man to put in charge, Jean Picard was selected. This was a natural choice, because in the founding of the Academy he was the representative of the astronomic side.

Picard deserves more than a short report of his geodetic work, since we are indebted to him for the establishing of that important serial entitled "Connoissance des Temps"; for making known to the world the principle of the micrometer, an attachment in the focus of the eyepiece of a telescope for measuring minute stellar distances and diameters; and for the attachment of a telescope to graduated arcs for measuring angles. Prior to Picard the unaided eye was used in sighting to distant stations, introducing uncertainties from poor definition, the

large angular opening of the eye, and the impossibility of accurately pointing when rude sights were used which of themselves subtended an arc of several seconds or even minutes. Therefore, when he aided vision by using a telescope and secured accurate bisection by placing in the telescope cross-lines whose intersection marked the optical axis, he made angular determinations a possibility, and advanced astronomy and geodesy far along towards its present stage of perfection.

At this time the earth was regarded as perfectly round, so when Picard undertook to obey his instructions he deemed it necessary to determine the length of *one* degree only, since all degrees must be of the same length. Following the plan of Snell, a base was measured. This was situated along the road between Villejuive and Juvisy, having its termini in a mill at the former place and a pavilion at the latter. The apparatus employed consisted of four rods of wood, each two toises in length, joined together two and two, making two bars four toises long. In measuring, the rods were placed on the pavement of which the road was made, end to end, and directly under a long chord stretched to test the alignment when laying the bars. The method of procedure was simple, and is in the main still followed with some forms of apparatus;

that is, one bar is placed in position, the other placed in line, with its rear end abutting against the forward end of the first bar, then the first bar is removed and is put before the forward bar, and so each bar is first in front, then in the rear, saving the time and avoiding the uncertainty of marking the end of the bar each time that it is applied in measuring. One will perceive that this plan requires two carefully adjusted units of length, and the exercise of great care in putting the forward bar in place, that it does not push the rear one backwards or otherwise disturb it. In order to keep count, each of the measures was provided with ten pins, one of which was dropped each time a bar was put in place, so that, as each pin represented four toises, eighty toises were measured when all the pins were expended. This will remind those who are familiar with it of the "stick," "stuck," of the surveyors of to-day, and if we had the naïveté of the French we might say that "the method of keeping count employed by Picard was the same as the one now in use by our county surveyors."

In this manner was the base twice measured, the first time giving 5,662 toises and 5 feet, the second, obtained in going in the opposite direction, was 5,662; as the latter value possessed the advantage of being a round number,

it was accepted as the length of the base. At the other end of the chain a base of 3,902 toises was measured to serve as a check on the triangulation.

The toise used in this work contained 6 feet, each foot containing 12 inches of 12 lines each. Picard, divining that his labors were to bear fruit of a lasting character, desired that his unit of length be more securely fixed than in a rod subjected to careless handling or even loss. He therefore constructed a pendulum making an oscillation in just one second, and ascertained its length in terms of the toise which he used. This he found to be equal to 36 inches 8.5 lines, and called it the *Rayon Astronomique*. One fourth of this length was to be the universal foot, and its double was to be the universal toise. This operation, which was prompted by precaution, has given all of Picard's critics a dangerous weapon, as they have a check on the standard which he used, especially when he added that the universal toise proposed by himself was to the Paris toise as 881 to 864. By a backward computation one can easily find what he considered the Paris toise.

He, however, cautiously suggested that these universal measures suppose that the change of localities causes no change in the length of the seconds pendulum; but, he continued, observa-

tions made at London, Bologna, and Lyons (where he met Mouton), seem to show that the pendulum beating seconds must be shortened in approaching the equator; still these observations are not sufficient to warrant this conclusion, especially as it is said that the seconds pendulum at The Hague is the same as at Paris. If it is found that the length of the seconds pendulum is different at different places, Picard realized that his universal toise would differ in every locality.

After this slight digression it will be in order to describe the continuation of his operations.

The angle-reading instrument was a quadrant of 38 inches radius; the stand was made of iron, while the limb bearing the graduation was covered with copper. There were two telescopes: one, whose optical axis was fixed, was the zero point, while the other, with its object end pivoted to the centre of the quadrant, moving over the limb, carried the index with which the angle was read. This duplex arrangement of telescopes enabled the observer to see that both objects were bisected at the same time; that is that the zero point was not disturbed in passing from one pointing to another. The movable telescope had attached to it immediately over the limb a small frame, across which, parallel with the axis of the telescope, a fine silver wire

was drawn to serve as the zero point of the index. The quadrant rested upon a stand provided with four foot-screws for leveling, and so mounted as to have an additional motion, permitting such tipping of the quadrant as was necessary to see the two stations observed upon when they were not in the same horizontal plane.

At each station the angles were read individually a number of times and by different persons, and the indiscriminate mean accepted as the correct value for the angle. After thus measuring each angle in the triangulation that was possible at the station, the precaution was taken to close the horizon by reading the angle between the last and first objects. It was found that in no case was there an error of closure, that is, the sum of the angles differing from 360°, of more than one minute, — a most excellent performance of an instrument whose smallest scale division was one minute.

The signals, sixteen in number, were in twelve cases tops of buildings, two were trees and two were made of dressed timber. In pointing to towers and large buildings, the angular diameter of the object was measured, so that the pointing to the middle could be corrected to either side, if it was found impossible to place the quadrant in the centre when occupying this station.

In two instances the angles were computed

from two known sides and one angle. In four cases the sum of the three angles did not equal 180°, having an error of from 10″ to 20″; this error was distributed arbitrarily, but with some regard to the uncertainties of bisection. It did not seem to occur to Picard that his triangles were spherical and that the three angles should slightly exceed two right angles, making the errors just mentioned somewhat larger.

In twelve of the triangles the two unknown sides were computed by different routes, in two instances giving identically the same results, while in the other cases the discrepancy ranged from one foot to five toises. When the computation was carried inward from the bases at each terminus of his arc, and united on a line in common, it was found that the values obtained for this line from these bases differed by four feet.

To determine the direction of any line of the triangulation Picard took his quadrant, and having removed one of the telescopes, mounted it so that it moved in a vertical plane. Then going to one of his stations, Mareuil, he pointed at the pole star at its elongation, and having bisected it, he let his quadrant stand till morning, and then located a distant point in the same vertical plane. This was repeated a number of times: then he says, "Considering that

the complement of the declination of the pole star at this time was 2° 28′, and the latitude of Mareuil 49° 5′, the azimuth of the pole star at elongation would be 3° 46′. The angle between this azimuth signal and the line to Cleremont was 4° 55′, hence the azimuth of this line is 1° 9′ west of north." This shows that he observed the star at eastern elongation. Combining the direction of this line to Cleremont with the angles of the triangulation, the direction of any line was easily found, and likewise the projection of each line upon the meridian.

The azimuth of the northern portion of the arc was determined in a similar manner during the following year. At this time the pole star reached its elongation just after sundown, so that the instrument was obliged to remain subject to accidental disturbances until the following morning before establishing the meridian mark. It is somewhat strange that Picard did not put this signal in place at night, because he had tried night signals, and speaks of the clear point which a fire on a distant signal gives, resembling in brilliancy a star of the third magnitude.

Having the lengths and directions of the sides, it was a simple matter to compute the projections of a set of lines which, joined end to end, would reach from one terminal point to the

other. Picard remarks that this method of computing the projection would not always give correct results, but that since in his chain the greatest azimuth was less than 19°, while the others were less than 3°, the sum of the projections, 78,907.5 toises may be regarded as the meridional distance between the terminal points.

The distance between the observatory at Paris and the parallel of Malvoisine was found, in a way similar to the above, to be 18,421 toises.

Although Picard observed with a compass the magnetic declination of each side, he congratulated himself upon the method he devised because he had noticed a change of 40′ in two years in the declination of the needle, and therefore feared the accuracy of any work in which data obtained from a compass entered.

The astronomic observations were made with an iron sector of ten feet radius, the limb covered with copper and divided into four-minute spaces. The telescope, also ten feet in length, was fastened to the sector, while the latter had a motion in the vertical. In the focus of the eye-piece were placed cross-wires, which were illuminated by having a light held near the object end. The readings were made from a plumb-line of fine silver wire suspended in a case ten feet long, having an opening just over the graduation. The error of the zero mark

was ascertained by turning the sector around to the other side of the vertical support, and taking the middle position of the plumb-line for the zero when the same object was observed. Great care was also taken to have the intersection of the cross-wires in the optical axis, which was also the centre of the objective.

The plan of observation was to mount the instrument in the meridian, and to measure the zenith distance of a star at the exact time it was on the meridian. This was previously computed, and the time taken from a well-regulated clock beating half-seconds.

The star selected was the knee of Cassiopeia, and a number of observations were made at Malvoisine in September, 1670, at Sourdon in September and October of the same year, and at Amiens in October. At each place the average of the observed zenith distance was taken, and as the range, or difference between the greatest and least distance, was very slight, he felt justified in taking the mean as correct. From these observations he obtained for the amplitude: —

From Malvoisine to Sourdon .	1° 11′ 57″
From Malvoisine to Amiens .	1° 22′ 55″

The positions of the astronomic stations increased the length of the first arc by 83 toises by actual measurement, and decreased the second by 57 toises, making for the distance: —

From Malvoisine to Sourdon . 68,342 toises,
From Malvoisine to Amiens . . 78,850 toises,

which combined with the amplitudes just stated would give for a degree 57,064.5 toises and 57,057 toises respectively.

Picard suggested that, owing to the time which elapsed between the times of observation at the different stations, a correction to his amplitudes should be applied, decreasing the first by $1''$ and the second by $1''.5$; but, fearing the charge of affected precision, he did not change his results.

The average of the two lengths obtained for a degree is 57,060.75; but he showed his weakness for round numbers by omitting the fraction and giving 57,060 toises for the length of a degree, asserting that this is correct within 32 toises, — the error corresponding to the neglected correction for $1''.5$.

Although Picard describes a level that he used, as well as a method of determining and allowing for refraction, and a table giving the difference between the true and apparent level, he makes no reference to any correction for angles measured in different horizontal planes, nor does he state the way in which he sought to avoid errors that would be made by measuring the angles on a circle not horizontal.

In comparing the work of Picard with that of Snell, one must remember that the former had

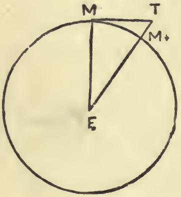
the advantage of logarithms to facilitate his work and to make check computations less laborious, while the telescope, invented in 1608, was nothing more than a toy at the time of Snell. Still the vantage ground gained for geodesy in this work of Picard may be summarized as follows: 1. The use of the telescope provided with cross-wires. 2. The measurement of a base-line by means of rods placed end to end. 3. The resting of triangles one upon another, forming a continuous chain without overlapping each other. 4. The projection of the sides directly upon the meridian. 5. Great refinements in the astronomic determinations.

In the light of modern criticism, Picard's results reveal a unique compensation of errors. He evidently used a false toise, as is seen from the ratio which he gives for his universal toise, deducible from the seconds pendulum, to the toise of Paris. His toise was to the correct one as 999 to 1,000. Then, while allowing for error of collimation arising from the deviation of the optical axis of his telescope from the centre of the object glass, he did not make any correction for aberration or nutations — sources of error which were not discovered until sixty years later. These errors, strange to say, were so nearly eliminated by mistakes made in the computations by Picard that a subsequent discussion of the whole

work gives for the length of a degree a value only 14 toises greater.

The labors of this Priest of Rillé, extending over the years 1669 and 1670, had a greater significance than is at first apparent, and will go down to posterity associated with the name of Newton.

Newton attempted in 1666 to prove his theory of universal gravitation by comparing the force of gravity at the moon's distance with the power required to hold the moon in her orbit. Omitting some of the details and using round numbers only, his method was as follows: —



Supposing the distance from E, the earth, to M, the moon, to be known, as well as the length of its orbit. The time it required the moon to move through its orbit was known, and hence the velocity at any point, as at M. This would give the distance through which the moon would pass in one second along the straight line M T, were it not pulled towards the earth by its attraction. Instead of being at T at the end of one second, the moon was at M_1, giving T M_1, as the fall of the moon toward the earth in one second under the action of the latter's attraction. Assuming Kepler's third law to be correct, the attraction

of the earth on a body would decrease inversely as the square of the distance from the centre of the earth. And since the distance through which a body falls in a given time is the measure of attraction, all the elements of the problem were in hand.

According to Newton's calculations, made at this time, the moon was deflected from the tangent every minute through a space of 13 feet, supposing the diameter of the earth to be about 7,000 miles. But knowing the force of attraction at the surface of the earth to be equal to 16 feet in one second, there was an error either in the distance from the earth to the moon, in the radius of the earth, or in the theory of universal attraction. Newton thought it was the latter, and "laid aside at that time any further thoughts of this matter."

But in June, 1682, at a meeting of the Royal Society, the result of Picard's degree-measurement was mentioned, and as it increased the earth's diameter about 1,000 miles, Newton made a note of this new value. He at once introduced his new factor in his problem, and as the solution drew near its close, it is said that he became so agitated that he was obliged to ask a friend to finish it.

The results showed that the deflection of the orbit of the moon from a straight line was equal

to a fall of 16 feet in one minute, the same space through which a body falls in one second at the surface of the earth. The distance fallen being as the square of the time, it followed that the force of gravity at the surface of the earth is 3,600 times as great as the force which holds the moon in her orbit. This number is the square of 60, which therefore expresses the number of times the moon is more distant than we are from the centre of the earth, — a ratio which Picard's value for the earth's radius established.

If, with the somewhat rude means employed by Picard, his errors had not eliminated one another, or if their extent had been discovered without knowing their compensating character, the undemonstrated law of gravitation would have remained as an hypothesis, celestial mechanics would have been without the mainspring of its existence, and we would now be groping in the darkness of an antecedent century.

It may be true that when the time is ripe for a great discovery, a competent man is raised up to announce and substantiate it. Kepler needed a force to hold his planets in their places — this he tried to derive from a supposed revolution of the sun on its axis, but he failed to see why this force should be unremitting, — its most essential feature. Galileo in his theory of falling bodies, and Huygens in his central forces, assumed pow-

ers that were without any visible analogue in nature. In Newton was the fruition of the ripe time: he recognized the force drawing an apple to the ground from a tree-top, a stone from the top of the loftiest structure, a drop of water from the highest cloud, to be the same as that which draws the moon to the earth, both to the sun, with an equalizing force to keep each in its place. But Newton did not regard an hypothesis as sufficient; it needed verification; so when, at the age of twenty-four, from inaccurate data his demonstration failed, he laid aside this theory, so brilliant in conception yet inefficient in action.

Had Picard announced his results forty-five years later, the ripeness of the time would have passed by with only Newton's failure to check the search for that grand essential theory without which we could have no exact astronomy, no celestial mechanics, no fluid motion. The French geometer stumbled more wisely than he knew; the English philosopher harmonized theory with fact, applied the finite to the infinite, and harnessed the worlds with invisible traces.

CHAPTER IV.

SOME THEORIES REGARDING THE MATHEMATICAL SHAPE OF THE EARTH.

In order to fully understand what follows, it will be necessary to stop long enough to give a few definitions, and call to mind some geometric relations.

All circles contain 360° : hence if in any circle we know the length of one degree, 360 times that quantity will give the entire circumference. As the circumference increases in proportion as the radius increases, the length of one degree will increase with the increase of the radius. Then since the longer radius gives a circumference of less curvature, — that is, more like a straight line, — the less the curvature the greater will be the length of one degree.

If a meridian of the earth is not a circle, suppose it is an ellipse. If an ellipse, which axis will be polar, and which equatorial? From the figure of the ellipse we see that the greatest curvature is at the extremity of the longer axis, therefore a degree at this point will be at the minimum, while the least curvature and greatest

degree are at the extremity of the shorter axis. If we know the length of a degree at the equator and at the pole, and find that the former is the shorter, we know that the equator is at the end of the longer axis, giving what is called an oblate or flattened spheroid; while, if the opposite conditions had been observed, a prolate or elongated spheroid would have resulted.

So long as the earth was regarded as spherical, the length of one degree sufficed to give its circumference; but as soon as more extensive geodetic operations were prosecuted, different degree-lengths would suggest a non-spherical earth. It was this condition of affairs, as well as theoretical investigations, that brought forth the spheroidal theories prolate and oblate.

In this connection the first name that suggests itself is Newton. To this modest Lincolnshire philosopher, born December 25, 1642 (O. S.), the world owes a great debt of gratitude for the impulse which his theories gave to physical sciences. Just when he first arrived at the conclusions which here concern us is not definitely known, but so far as priority is concerned, the date of the publication of the "Principia," 1687, is sufficient. He so carefully laid down principles and so accurately sketched out the proper view to be taken of every part of the subject, that his followers have done little else than fill up his original

outlines. The modern theory of the figure of the planets, still imperfect in some respects, coincides in the main with the physical ideas of Newton. The progress of the mathematical sciences has enabled the philosophers since his time to develop and extend his theory.

It is supposed in the "Principia" that the earth is a mass of homogeneous fluid, the particles of which attract one another in the inverse proportion of the square of the distance. If there were no motion of a rotary character, the only figure consistent with the equilibrium of the attractive forces would be a perfect sphere. But as the earth revolves upon an axis, a centrifugal force is communicated to the particles of the fluid, causing them to tend to recede from the axis, and changing the sphere into a figure oblate at the poles and protuberant at the equator.

The ratio of this centrifugal force to gravity was easily found. Every point of the equator describes in a second of time a circular arc, as A B, in which the versed-sine A D, the vertical component of the centrifugal force, is equal to 0.67 of an inch, which is nearly $\frac{1}{288}$ of $16\frac{1}{2}$ feet, the space through which a body falls in a second of time. Hence the centrifugal force is $\frac{1}{288}$ of the observed gravitation, or $\frac{1}{289}$ of the attractive

force that would prevail if the earth preserved its figure at rest.

In the question of the figure of the earth, we may therefore suppose a sphere consisting of a homogeneous fluid at rest, and consequently in equilibrium, and we may inquire what change would ensue in consequence of a rotary motion causing a centrifugal force very small in proportion to the gravity. Newton tacitly assumed that the fluid sphere, in the nascent change of its form, will become a spheroid such as is generated by the revolution of an ellipse about the less axis, and that the meridians are ellipses. His determination of the figure of the earth is liable to objection in assuming that the fluid sphere changes into an oblate elliptical spheroid by the action of the centrifugal force. It is also defective in considering only the extreme case of a fluid mass perfectly homogeneous.

Inseparably connected with the name of Newton is that of Huygens. His attention was first drawn to the subject of the figure of the earth by the variation in the length of the seconds pendulum in different latitudes, which was first announced as an observed fact when Richer returned in 1672 from Cayenne. It was quite natural that Huygens should take notice of everything related to pendulum behavior, as he was at this time busy with the application of the

pendulum to the regulating of clocks. He immediately perceived that this phenomenon — the shortening of the seconds pendulum on approaching the equator — was caused by the centrifugal force at the earth's surface, which, increasing as the equator is approached, lessens the power of gravity and retards the time of the pendulum's vibration. It also occurred to him that if the earth were a perfect sphere, a plumb-line would not be at right angles to the sea, or to the surface of standing water, but would be deflected somewhat by the action of centrifugal force. Hence a light body in still water would not press perpendicularly upon the surface and consequently could not be at rest, which is contrary to experience. Huygens therefore argued that the earth was not spherical, but protuberant at the equator, so that the terrestrial meridians might everywhere be perpendicular to the plumb-line. Here he ceased speculating, perhaps waiting for observations to in some way substantiate his views, nor did he proceed until Newton furnished the stimulus in the nature of a suggestion.

Newton determined the ratio of the two axes by conceiving two columns of fluid to extend from the centre of the earth outward towards the surface: one to the equator, the other to one of the poles. Since these two columns were in equilibrium they would press upon one an-

other with equal intensity, so that the ratio of their lengths would be found by comparing their weights. The weight of the equatorial column is equal to gravitation diminished by centrifugal force, while the polar column, unaffected by the earth's diurnal motion, had a weight dependent solely on the gravitation of its particles.

This centrifugal force of each equatorial particle depended upon its angular velocity and its distance from the centre, but its gravitation, resulting from the combined attraction of the surrounding particles, was one of the problems first solved by Newton. The result of this discussion gave as the ratio of the polar to the equatorial axis, 229 to 230. This is usually stated $b : a :: 229 : 230$, and the ellipticity, which is the ratio of $a-b$ to a, is $\frac{1}{230}$.

This was the needful suggestion for Huygens. He at once took up the problem, but rejecting the Newtonian principle of an attraction between the particles, he placed in the centre of the mass a force attracting the particles according to the inverse square of the distances. Upon this hypothesis he proved that a homogeneous body of fluid revolving upon an axis will be in equilibrium when it has the figure of an oblate spheroid very little different from a sphere, the ellipticity being one half of the ratio of centrifugal force to the gravity at the equator, or

$\frac{1}{2} \times \frac{1}{289} = \frac{1}{578}$. He tried to verify his theory from actual experiment, so he took a soft globe of clay and attached it to an axis. This he caused to revolve very rapidly, when he observed that the ball became flattened at each end of the axis and enlarged at the middle.

He very cleverly concluded from this theory that the water adjusts itself over the surface of the earth, and that the fixed land must do the same, — for if the land at the poles were exactly at the same distance from the centre of the earth that the equator is, the water of the sea in this latter region would be raised above the land. But since we find at the equator large areas of land, these too must have been thrown out under the action of centrifugal force, and for this to be possible, the earth was at one time in a fluid condition, becoming solid later in its life. Quite recently Professor Stokes has enunciated almost the same idea when he said that the fact that the waters of the earth are in equilibrium shows that at some time there must have been a bulging out of the land in the equatorial regions. Newton saw in the flattening of Jupiter as reported by Cassini (1691) a proof by analogy of the oblate hypothesis.

Cassini, who will be referred to at length in the next chapter, did not see in the change of length in the seconds pendulum at Cayenne any

cause beyond the action of increased centrifugal force at the equator. This occasioned Newton to examine the effect of this force at that point and to compare it with the similar force at Paris. He showed that, allowing for these unequal centrifugal forces, the difference in the lengths of the seconds pendulum was too great for a spherical earth.

About forty-eight years after the publication of the "Principia," James Stirling communicated to the Royal Society of London two propositions in which he proved the correctness of Newton's assumption that the oblate spheroid is the condition of equilibrium of a revolving homogeneous fluid. In 1740 Maclaurin, in his prize essay on the "Tides," proved that a homogeneous fluid mass, having the form of an oblate elliptical spheroid, will be in equilibrium when it revolves upon its axis in a proper time. Supposing the attractive forces to act at every point of the spheroid, he determined the rate of diminution of gravity from the pole to the equator and the relation between the ellipticity and the centrifugal force. His mode of attack was by the classical method of pure geometry, and so powerful was this implement of research in his hands that when Clairaut took up this problem he abandoned analysis and reverted to geometry. The investigation of the latter went farther than

those of his predecessors, and in his treatise on the figure of the earth published in 1743 he extended Newton's hypothesis so as to include spheroids of different densities. He reasoned that since each particle is thrust towards the centre of the earth by a force equal to the weight of all the overlying particles, the density of the earth must increase towards its centre. If this is the case the ellipticity of the earth will lie between the values given by Newton and Huygens.

It is in this paper that one finds the proof for the celebrated formula for the accelerating effect of gravity in a place whose latitude is l: —

$$g = G[1 - (\tfrac{5}{2}m - e)(\tfrac{1}{3} - \cos^2 l)]$$

in which G is the value of equatorial gravity, m the ratio of the centrifugal force to gravity at the equator, and e the ellipticity of a meridian of the earth. This formula has its special importance in making it possible to use the length of the seconds pendulum in determining the earth's figure.

In his subsequent elaboration of his investigation Clairaut left the subject of the figure of the earth practically as we now find it, so far as the theory is concerned. "The splendid analysis which Laplace supplied, adorned but did not really alter the theory which started from the

creative hands of Clairaut." In Clairaut's demonstration the earth is supposed to be made up of concentric strata, ellipsoidal — differing only slightly from a sphere — and revolving round a common axis. Each stratum is homogeneous, but may differ in density from the contiguous strata by any law whatever, or even change densities discontinuously. He did not assume, as many think, that the strata were originally fluid, but merely supposed that the superficial stratum has the same form as if it were fluid and in relative equilibrium, when rotating with uniform angular velocity. Nor did he define any limit to the law of the variability of the ellipticity in passing from one stratum to another, except that the ellipticity be continuous, and at the surface must be such as would correspond to the relative equilibrium of a film of rotating fluid.

The investigations here referred to were prosecuted purely in the interest of science and with a desire to add to the world's stock of knowledge, and not with the wish to defend or controvert the results of degree-measurements. These were obtained at intervals during the fifty years in which the theoretical side was receiving attention. Some of the results apparently upset the theory, while others upheld it.

And while narrating the operations which followed those of Picard, the testimony which each offers in the question of oblate or prolate spheroid will be mentioned.

CHAPTER V.

CONTINUATION OF PICARD'S WORK.

When Newton announced his belief regarding the shape of the earth, and what seemed to him a conclusive proof that it was oblate, the French savants, thinking that France was the royal arcanum of the sciences, and that England was a Nazareth from which no good or true thing could come, looked with impatience upon the interruptions which befell the continuation of Picard's arc.

Cassini the elder proposed the extension of the work begun by Picard until it should reach from the most northern to the most southern extremity of France. The proposition was well received by Colbert, the liberal minister of Louis XIV., and work was ordered to proceed at once. The plan of Cassini contemplated an arc of about eight degrees, whose axis should be the meridian of the Paris Observatory. Accordingly he sent his associate, De la Hire, to begin work at the northern end of the arc, while he himself made a commencement at the southern end. But operations were soon interrupted

by the death of Colbert, and the wars which followed prevented a resumption of the work until the year 1700. During this year the king's attention was called to the importance of this undertaking. Cassini was ordered to resume operations, which he at once did with the assistance of his son Jacques and Maraldi, Coupler, and Chazelles.

The work began at the southern extremity of Picard's arc, and was carried forward until the southern boundary of France was reached. The reductions seemed to show that the length of degrees increased towards the equator, agreeable to the theory which J. Cassini had announced in 1710. It is true that the increase was very slight, only 11 or 12 toises in each degree, which at that time was regarded as probably owing to inaccuracies in different parts of the work. It was deemed necessary to measure degrees widely separated in order to definitely settle the question of increase in the length of degrees ; so Cassini, under royal instructions, decided to continue the arc northward, and regarding the triangulation of Picard as beyond reproach, he affixed his triangulation to it. In a short time the arc was carried as far as Dunkirk, whose longitude was only 2′ 20″ east of Paris, while the southern end was at Perpignam.

The latitudes were determined from observations of zenith distances made with a sextant of 10 feet radius, graduated to 20″, and read by approximation to 5″. The results of these observations gave 6° 18′ 57″ for the amplitude of the southern arc and 360,614 toises for its length, while the northern end had an amplitude of 2° 12′ 15″, and a length of 125,454 toises. The whole arc gave 486,156 toises for 8° 31′ 12″.

On the beach near Dunkirk a base line was measured twice. The accepted length was 5,564 toises, while the individual values differed from one another by one half toise. This length likewise differed from the length obtained by computation from Picard's base by one toise. The success attained in the base at Perpignam was considerably greater, or the errors were of a compensating character.

The results of these arcs gave :—

1° of the southern arc	57,098 toises.
1° of Picard's arc	57,060 toises.
1° of the northern arc	56,960 toises.

From this it would seem that degrees increase in length towards the equator : hence the earth is prolate, or contrary to the hypothesis of Newton.

These values, published in 1718, received during the next few years several revisions, which showed so many weaknesses that they

were by no means convincing in deciding against the oblate hypothesis, but they added to the sharp discussions which prevailed for fifty years after the publication of the "Principia." At this time the scientific men of Europe were divided into two factions upon this question: geodetic data on the one hand pointed towards the prolate theory, but the oblate theory evolved by pure mathematics found its first demonstration in the behavior of Richer's pendulum at Cayenne, — an isolated fact soon confirmed by Halley at the Island of St. Helena, Varin and Des Hayes at Martinique, St. Dominique, and Porto Bello, and Gorée upon the coast of Africa.

The French Academy, piqued at the inability of their work to convince mankind, desiring to overthrow views of English origin or actuated by a determination to settle once for all this mooted question, decided to measure a degree at the equator. The Academy did not possess the means to equip the expedition, but through the intercession of Maurepas and Cardinal Fleury Louis XV. made the requisite grant. Consequently on May 6, 1735, the party, consisting of Bouguer, De la Condamine, Godin, a number of mechanicians and servants, together with several Spaniards, amongst them Gorg Juan and Antonio Ulloa, set out for Peru. It is said

that ill-health prevented Grandjean de Fouchy from taking the post of honor in this expedition to which he had been assigned.

Maupertuis, born at St. Malo in 1698, left the Dragoons, of which he had been captain, to devote his time to mathematics and especially to astronomy. It was he whom Frederick the Great invited to come to Berlin to "put the Academy of Sciences into shape, to graft into this wild crab-tree the graft of the sciences," saying, "you have shown the figure of the earth to mankind, show also to a king how sweet it is to possess such a man as you." He at least succeeded in grafting into the Academy the French language, which explains why the early memoirs are published in French.

This military astronomer impressed upon Maurepas the great importance of having the length of a degree at some point north of France, and succeeded in having an expedition fitted out to go to Lapland for that purpose.

The party, made up of Maupertuis, Clairaut, Camus, Le Mounier, Sommereux as secretary, and Herbelot as designer, sailed from Dunkirk on May 2, 1736, and though the time of departure was a year subsequent to that of the Peru expedition, the results of the former were communicated to the world at large so much sooner than those which came from the equatorial arc

that the work in the north demands the attention first.

Maupertuis, judging from the numerous islands which the map showed in the Gulf of Bothnia, expected to measure a base along the shore of the gulf and expand his chain of triangles by selecting points near the coast and on these islands. But a careful examination of this locality revealed the fact that the islands were not suitably situated to be of any service in the triangulation. This state of affairs suggested a trip inland in search of ground fit for base-measuring, and points or mountains well situated for stations.

At Upsala, Celsius the Swedish astronomer joined the party, as did also two young Swedes. These rendered most valuable service while traveling through a country in which only their language was understood.

The point now in view was Tornea, a small town on an island in the northwestern corner of the Gulf of Bothnia. This town was situated at the mouth of a river of the same name, which in its southward course passed through a valley skirted by two parallel ranges of mountains. The latter promised good situations for the triangulation signals, but a straight stretch of level ground for the base was not so easily found. Clairaut proposed that they wait until

winter and measure this line on the ice frozen over the river. This plan appeared so feasible that a reconnoissance was at once begun. The river formed a most opportune means of travel, so on July 6 the party set out in boats up the river accompanied by several Finnish soldiers. The journey was attended by many hardships — wading through marshes which in many places skirted the river, penetrating the thick undergrowth around the foot-hills, and climbing precipitous mountains incessantly pursued by gadflies of the most tormenting kind.

Upon each point which was found available for a station they built signals, of trees stripped of their bark, in the shape of hollow cones. These barkless poles rendered the signals visible at a distance of ten or twelve leagues. Under the centre of each signal was placed beneath the surface of the ground a large stone with a mark on its upper surface to indicate the centre. This was to enable a signal to be replaced in its exact position in case it should be blown down between the time of beginning and finishing observations upon it.

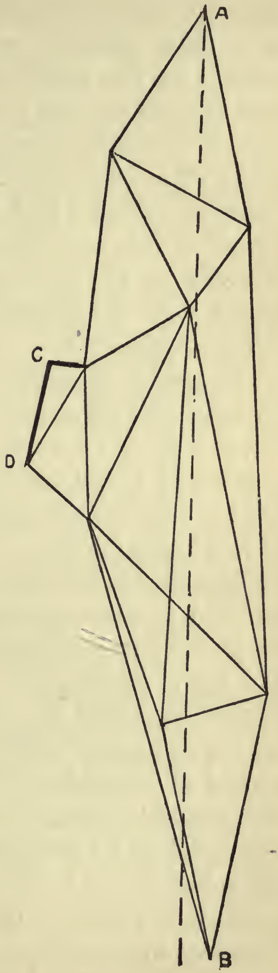
For measuring the angles they used a quadrant of two feet radius, made by Langlois and provided with a micrometer for reading the angular distance from the zero point to the nearest subdivision on the arc. With this instru-

ment each of the observers read all the angles at a station and made his own record; then the average of the different recorded means was taken as the correct value of the angles. At this season of the year the fogs were so frequent that it was often necessary to wait several days at a station before making any observations, but it appears from the records that all the readings were made, when once begun, without disturbing the quadrant. Vertical angles to the other signals were measured at each station in order to determine what correction would be necessary to apply to reduce the angles to their equivalents in the same horizontal plane. The effort was always made to place the instrument over the centre of the station, even if this position should necessitate a cutting out of sight lines, as was the case at Mt. Kukas, where lines to all the other stations had to be cut out. In one case an eccentric position was occupied by the quadrant and a reduction to centre was made, but the formula for computing this reduction is not given.

It is somewhat difficult to know what accuracy was attained in measuring the horizontal angles. In one case only is there given two individual angles and their sum, and here the single angle exceeds the sum of its parts by $27''.4$. The errors of the triangles, that is the amount by which the sum of the angles differs from 180°,

varied from 2″.9 to 29″.4. These were corrected arbitrarily until the angles of every triangle gave exactly two right angles. The entire figure formed a heptagon, the sum of whose perimetal angles should be 900°, while the sum of the 16 angles which made them exceeded this geometrical limit by 1′ 37″.

Azimuth was determined by observing the angle between the sun when upon the horizon and one of the signals. This was done at both ends of the arc, and the directions of all the lines were ascertained from these terminal



A, Kittis; B, Tornea; C D, base-line; the dotted line, the meridian of Kittis.

azimuths, considering the triangles as situated on a plane surface. A discrepancy of several minutes in the two directions thus found for each line caused the participants great uneasiness, until Clairaut suggested that the convergence of meridians would cause a difference, especially in such a high latitude. He computed the effect this convergence would have on the azimuths at the two ends of the arc, which agreed with the observed difference to within half a minute.

The amplitude of the arc was determined by obtaining the difference in the latitudes of the terminal points; for this purpose they employed a sector of 9 feet radius, made by Graham of London. It was mounted on a heavy pyramid 12 feet high; the arc of $5\frac{1}{2}$ degrees was read by a micrometer microscope. The instrument was not used to determine the absolute latitude of the place by measuring the zenith distance of a star, but to ascertain the *difference* between the zenith distances of the same star at two places, and the consequent *difference* of latitude. This method of ascertaining the amplitude has been disregarded by those who refer to the "latitudes observed," and "latitudes re-observed." The star selected was δ Draconis, which passed the meridian very near to the zenith of both places. Observations were begun at Kittis on September

30, and continued for five days, yielding satisfactory results, — the greatest range in the zenith distances being 3″. The clock from which the time of culmination was taken was regulated each day by equal altitude of the sun observations. The sector was then set up at Tornea, and observations were made on the first five days in November.

These observations were corrected for aberration, precession, and the inequality which had recently been announced by Bradley, now known as nutation; but no correction was made for refraction, giving as a final result for the amplitude 57′ 26″.93.

These operations could in no wise give any intimation of testimony in favor of either the prolate or oblate hypothesis. The length of the arc hung in the air, and could not become known until after the measurement of a base-line.

In order that the results obtained by the parties of Maupertuis and Bouguer might be directly comparable, Langlois made two toises of iron, one for each party, and since then known as *toise du Nord* and *toise du Pérou.* It has been affirmed that these did not agree with one another, nor with the standard from which they were taken.

As this standard could not be used in the measurement of the base, Maupertuis had two

large nails driven into the walls of a room so that their heads were less than a toise apart. The temperature of the room was then reduced by a fire to 15° Réaumur, the temperature at which his toise had been compared with that of the châtelet at Paris. Then the heads of the nails were filed down until the toise could exactly come between them. Five pieces of wood, tipped at each end with a round thimble of metal, were so adjusted that they would just come within these two nails. After this, eight pieces of fir were procured, approximately five toises in length and tipped with iron. These were adjusted so as to fit between two nails which exactly contained the five individual toises when placed carefully end to end and in line. The rods used in measuring were each day placed within this comparator, and no change was noticed in their lengths.

The base-line was measured on the frozen surface of the river Tornea, very nearly in the direction of the stream; the two extremities of the base were on land, and on opposite sides of the stream. The measurement began on December 21, and required seven days for its completion, because of the shortness of the days at this season. There were two parties, each provided with four rods: these they laid end to end on the snow, and aligned by placing them under a cord

drawn taut from pegs carefully placed in line. Both parties measured in the same direction, and the same number of rods each day, so that at the end of a day's work the difference in their measurements was noted; this difference never exceeded one inch, while the total difference in the two measurements of the entire base was four inches in 7,406.86 toises.

To verify the total number of toises found in measuring, the entire base was measured with a cord 50 toises long; two lengths of which should reach from one stake to the next that had been placed in measuring at every hundred toises.

The discomforts were very great during this work. The snow was so deep that it was necessary to make a partial path, which was done by hitching a horse to one of the angles of a triangle made of strong boards, and driving him over the course. Notwithstanding this, the depth sometimes remained at two feet. Nearly every one had his hands and feet frozen, the cold being so intense that water would freeze while putting it to one's mouth, so that the only liquid that could be drunk was brandy.

The close agreement of the two values for the length of the base is not at all remarkable, as the two measurements were executed at precisely the same time and under the same conditions of temperature. Maupertuis had suspected that the

wooden rods might be susceptible to a change in length on account of a change in temperature, so he took the precaution to place his rods in the comparator when the thermometer showed widely different temperatures. As the contraction did not appear to equal the thickness of paper, he concluded that wood did not contract as did metals with cold.

It is not known that he was ignorant of the necessity of reducing his base to its equivalent at sea-level, but, at all events, the surface of the water of this river, not very far inland, could be only a few feet above mean-tide. However, he did not realize that when laying a long rod, if it were not level, he was measuring the oblique distance, the hypothenuse, while the projection or base was wanted.

The inclemency of the winter, or a desire to test their work somewhat while still near, to repeat any observations that might be uncertain, prompted Maupertuis to wait at Tornea until early spring. In the mean time the calculations were made, giving 55,023.47 toises for the distance between the parallels of Kittis and Tornea as the average of two computations which differed from one another by 4.5 toises. This result gave for the length of a degree a value 1,000 toises larger than it should have been, according to the theory of Cassini. It was also too large

for the formula of Clairaut, so it was decided to verify their work. The base was considered to be above reproach, and the agreement of the individual values for the angles as well as their accord with the geometric requirements emphasized the belief that the geodetic side of the work was strong. They therefore resolved to re-determine the amplitude, and for this purpose they selected α Draconis, observing at Tornea on March 17, 18, and 19, 1737, and at Kittis, April 4, 5, and 6. The result when corrected as in the former case was $57' \; 30''.42$, which exceeded the first amplitude by $3''.49$.

This discrepancy in the amplitudes suggested some defect in the construction of the sector, which they determined to test. They measured a distance of 36 toises 3 feet 6 inches $6\frac{2}{3}$ lines, and from the middle of this line and perpendicular to it another of 380 toises 1 foot 3 inches. At the extremity of this line the angle which the first line subtended was carefully measured with the sector. The average of the results of five observers was $1''.05$ less than the computed value of this angle. Then upon examination he found that a degree of the arc used in measuring the zenith distance δ was one second greater than a degree of the part used in observing α, thus reducing the difference in the corrected amplitudes to $2''.5$.

Maupertuis did not seem to consider the error of great importance, so he took for the amplitude of the arc $57' \, 28''.65$, which gives 57,438 for a degree in latitude $66° \, 20'$.

The work being finished, the party started for Stockholm June 9, 1737; four days later their vessel sprung a leak, and was beached in order to save the lives of those on board and the instruments. The instruments were very wet, and eighteen months later, when cleaned, they were found to be very rusty, and no comparison or examination was deemed, under the circumstances, worth the trouble.

It is easy to imagine that the announcement of this result to the Academy on April 16, 1738, caused a great sensation. It controverted the views of Cassini, father and son, the greatest astronomers of their time, and upheld the Newtonian theory. In fact it was too vigorous in its upholding, since it gave in comparison with Picard's arc $\frac{1}{178}$ for the ellipticity — a value larger than the followers of Newton wanted.

In the years immediately following the return of Maupertuis — who as Voltaire said, had flattened the poles — and the Cassinis, he was under the constant fire of critics. They assailed him because he had neglected refraction, omitted the correction for bad division of the sector, disregarded the shortening of his rods in low temper-

atures, and because of numerous other points of weakness which were thought to exist. But when each and every one applied his correction, the length of the degree did not appreciably shorten. Then Maupertuis turned his attention to the arc of Picard. He applied corrections which Picard had ignored and lengthened his arc by 123 toises. But this was not enough: the earth was still too flat.

When the equatorial party returned, Bouguer and Condamine joined in the attack of the polar scientists, and received in turn the criticisms of Maupertuis.

The principal defect in this work was the shortness of the arc, a defect which was partly corrected by Svanberg in the first years of this century, an account of which will be given in its proper place. However, Maupertuis from pendulum observations strengthened the oblate hypothesis, as he found the seconds pendulum at Pello 0.6 lines longer than at Paris, while at the latter place the number of oscillations in twenty-four hours was fifty-nine less than at Pello.

The endless discussions and numerous reflections on his work caused Maupertuis to make plans for a re-determination of his arc at his own expense. He went so far as to correspond with Celsius on this subject, and would doubtless have secured his coöperation had not the

death of Celsius intervened. Maupertuis's ardor waned after the reception of Frederick's flattering invitation to come to Berlin.

To the lasting credit of this party it must be said that absolute harmony as to methods and details prevailed. All the members were accomplished mathematicians, though only Outhier had previously distinguished himself as an observer. Montucla ascribes the choice of leader to the possession of attainments other than scientific when he says: "Maupertuis était agréable, il faisait chansons, il jouait de la guitare et cela lui aida à obtenir la commission, qu'il demandoit." However this may be, a very large part of the success of the party was due to Maupertuis, as is shown by the high esteem in which he was held upon his return to France. One could see at that time in many of the shop-windows in Paris a picture of Maupertuis dressed in the costume of a Lapland Hercules, holding in one hand a club while with the other he compressed a terrestrial globe.

Notwithstanding the shortness of this arc, it has contributed data for all of the principal discussions regarding the shape of the earth deduced from geodetic data.

In discussing the equatorial work it is necessary to begin at the very point of embarkation. Godin first proposed that the king send an ex-

pedition to the torrid zone, there to measure an arc of the meridian, and also an arc of the equator. De Fouchy and La Condamine prepared at once to accompany him, and assisted in securing the indorsement of their plans by the Academy, and its adoption by Louis XV. It was proposed to measure both arcs in the neighborhood of Quito; this prompted Philip V. to give them the necessary permit, to command the Viceroy of Lima to render them all possible assistance, and to detail two Spanish officers to accompany the expedition.

When all preparations were complete, De Fouchy saw that his health would not allow him to undertake the fatigue of the enterprise and reluctantly withdrew. This left the party without a chief. Instead of giving this position to Godin, who had been the originator of the plan, Bouguer was selected, just why, it is difficult to imagine. He was, it is true, a member of the Academy, but living at Havre he seldom took part in its deliberations and did not seem to take any special interest in the project until after he was designated chief. While on the way he looked into the subject and came to the conclusion that an arc of the meridian was all that was needed, and that this should be located, not at the equator, but near the shore north of Quito.

It has been deemed advisable to make this introductory explanation, so that one may understand why this work occupied so many years and why, subsequent to their return, the members of the party seemed to be at war with one another. It was only natural that there should be some discord when the general direction was given to one who had nothing to do with the inception of the work, and who insisted upon a plan of procedure at variance with that of the proposer and those who were in sympathy with him.

The expedition set sail on a naval vessel from Rochelle on May 16, 1735, and arrived at Carthagena on November 16, where the two Spanish officers who had been waiting some months joined the party. Panama was reached December 29, after having crossed the Isthmus from Porto Bello. The coast of Peru was sighted March 9, 1736, and on the following day a landing was made, and the village Monte-Christi, situated about three leagues inland, was selected for temporary headquarters. Their experience in transporting instruments and the baggage of twenty-six persons across the Isthmus caused Bouguer to hesitate to undertake the journey inland as far as Quito. While deciding just what course to pursue, nearly three months of valuable time had slipped by; however, he spent a

part of it in observing for astronomic refraction, trying to alienate La Condamine from Godin, and in securing from France an order that the latter should not carry out his pet idea of measuring an arc of the equator. Quito was finally reached on June 10, 1736, more than a year after their departure from Europe. After seeing how well suited this section was for triangulation, Bouguer makes no reference to his violent opposition to leaving the coast.

After deciding upon the general direction of the chain, the first work which engaged the attention of the party was the measurement of a base-line. This was located near the village Yarouqui, about fifteen miles east of Quito. At this point the confluence of several mountain streams formed a valley of considerable extent, but still it required several days to find the most favorable site. The requirements of a base-line as they were conceived at that time were: that both ends be visible from all points, that the termini be favorably situated with respect to at least two of the triangulation points, and that the character of the ground be such that an accurate measurement might be made over its entire extent. The only addition that modern practice has made is that the profile of the base shall not include inclinations exceeding a definite angle, say 2° or 3°, and that the length

of the base need not be longer than the sides of the triangle of which it forms a part.

After finding a stretch of ground satisfying the greatest number of conditions, it required eight days' work grading and removing obstacles before the measuring could begin. For this purpose the men were divided into two parties, under the leadership of Godin and Bouguer. Each party was provided with three wooden rods, twenty feet in length, so that when all three rods were in position ten toises were measured. These wooden rods were provided at each end with a projecting piece of copper. At one end this plate was horizontal, at the other end it was vertical, — consequently when two rods were brought into contact the ends touched in one point only. This was a great improvement on the blunt ends previously used, and was a step towards the knife-edge contacts subsequently introduced. The iron toise which had been brought from Paris and afterwards known as the *toise du Pérou* was carried along, always kept in the shade; with this the rods were compared daily. Alignment was effected by stretching a cord from stakes previously put in line by sighting. Owing to the violent winds which occasionally would come down the ravines, it was found necessary to lay the rods on the ground. In measuring, two rods were always

left in position, while the third was carried forward and brought into contact with the front rod as carefully as possible in order to avoid displacement from shock. The ends were brought into the same horizontal plane for contact by raising or lowering the foremost bar by means of wedges. After seeing that this bar was in position for making contact, the inclination was ascertained by placing a graduated level on the bar.

This was necessary, since the length of the rod was the hypothenuse, while the length wanted was the projection of the rod, or the base of the triangle whose hypothenuse was the rod. The slighter the inclination the nearer this projection would equal the length of the rod, that is, the projection is a function of the inclination.

When the character of the ground was such that the rear end of the foremost bar could not be brought into the plane of the forward end of the adjacent bar, these ends were brought into the same vertical by holding a plumb-line against which the two ends should abut, thus having the one end directly under the other. As the ends were on opposite sides of the plumb-line, the thickness of the cord should have been allowed for each time this operation was employed — a precaution that was neglected.

Upon making a halt for the night, two pegs were driven at right angles to the direction of the line and as nearly as possible in the vertical of the forward end. A plumb-line was then held from the end of the bar and a thread drawn from the tops of these stakes was moved until it was directly under the point of the plumb-bob. The exact position of the thread on each stake was then marked. When work was resumed on the following day, the thread was put in place and the rear end of a bar was placed against it or in the same vertical — a method that is still used in some species of work.

The two parties followed the same general plan, but began at opposite ends of the line. When they met near the middle of the line, they found by comparison that their rods were of the same length.

During the entire work changes in the lengths of the bars were ascertained by the comparisons just referred to, and allowances were duly made. The accuracy with which these corrections were made, or the success with which all errors destroyed one another can be seen from the statement that the difference in the length of the base as found by the two parties was less than three inches in about 6,273 toises, or an error of 1 in 150,000. The length after reduction to the level of the northern end, and after applying some

revised corrections was 6,274.057 toises, as found by Bouguer, and 6,274.045 by La Condamine.

The triangulation extended from this base southward through 32 triangles to a point near Tarqui, where Bouguer in 1739 measured a second base. The general plan pursued in measuring this base was the same as the one employed on the first. La Condamine, however, had charge of one party. He introduced one new feature, that is, whenever it was necessary to drop a vertical by means of a plumb-line to the ground, when stopping for the day, or bring the rear end of one bar and the forward end of another into the same vertical, he measured the distance from the bar to the ground or the distance from one bar to the other. The algebraic sum — considering " up " as positive and "down" as negative, would give, together with the sum of all the rises and depressions made by the rods in not having them level, a check on the difference in elevation of the two ends of the base as determined by zenith distances.

The ground at Tarqui was better suited for a base than at the northern extremity of the arc, as it was almost a perfect plain, intercepted, however, by a pond in which there was about one foot of water. Through this pond stakes carefully put in line were driven into the bottom of the pond at a suitable distance apart. The

rods were allowed to float while being brought into contact, and were then firmly held in position by being lashed to the stakes. During the measurement frequent comparisons were made with iron toises, one of which was carried by each party.

Upon comparing the results of the direct measurements it was found that the difference was only four lines. But when each observer applied the corrections which he deemed necessary, and reduced this base to its equivalent at the elevation of the north end of the Yarouqui base, the difference was increased to seven lines.

This pond was a most important factor in the reduction of the length of the base. It gave the approximate temperature, somewhat indirectly, it is true, but La Condamine did not hesitate to say how he used this thermometer. He says that in the 600 toises which they measured across this pond he was in the water up to his knees, and as he experienced no more discomfort than when he had been in water at a temperature of 20°, he concluded that the average temperature during the measurement was between 16° and 17°. As 10°.5 was taken as the average temperature of Yarouqui base, the mean of this and 16°, or 13°, was assumed as the temperature to which both measurements should be reduced. This being the temperature at which

the toise had been originally standarded, La Condamine did not see any need of making any temperature correction in comparing the measured with the computed length of the second base.

Just here it might be remarked that La Condamine was perhaps the first to make accurate observations for the determination of the coefficient of expansion. These experiments, made upon his return to Paris, were with the *toise du Nord.* This and another authenticated toise he converted into pendulums and allowed them to swing in two separate rooms, but so situated that he could observe both at the same time. Both rooms were brought to a temperature of 13°, and the number of oscillations made by both pendulums in a given time was counted. He then had the temperature of the room in which the *toise du Nord* was hung raised to 55°, while that of the other room remained at 13°. By again counting the number of oscillations made by both in a given time, it was easy to compute the amount of lengthening caused by raising the temperature through 42°. This, when reduced to our units, would be expressed by .0000058 for 1° F., a result which compares very favorably with more recent determinations.

The angles were measured with different

quadrants in the hands of three different observers. La Condamine's had a radius of three feet, Bouguer's about two feet and a half, Godin's not quite two feet. Each was provided with two telescopes, one fixed and the other movable about the centre of the quadrant, while the whole instrument possessed universal motion through two cylindrical elbows placed at right angles to one another. Micrometers were attached to each quadrant — antedating in construction but not in use the micrometers employed by the Polar party.

We are indebted to Bouguer for suggesting a systematic study of instrumental errors. He spent an entire month examining his quadrant, subjecting each five degrees to a rigid scrutiny by measuring an angle accurately known from the geometric relations of its sides. He also suspected another source of error, now called error of eccentricity, arising from the non-coincidence of the centres of rotation and graduation. These errors of graduation and eccentricity were determined and tabulated so that the correction could be at once applied to each angle. All angles were reduced to the same horizon, and in this computation the principles of spherical trigonometry were employed for the first time in geodetic work. In reading the angles at each station the horizon was always closed, the sum

of the measured angles when corrected gave a result differing from 360° by 2′ on an average. The angles of a triangle usually required an arbitrary correction of about 20″ to bring them into accord with the geometric requirement.

The triangulations of Bouguer and La Condamine were identical, but Godin switched off after occupying about half of their stations and brought his work to a close at a different place with a measured base at the end. Just what success he met with is not known, as he never published his results nor left manuscripts from which they could be gathered. As Juan worked with him, it may be that he incorporated their joint results in his book published at Madrid, 1748.

At this time it was not considered necessary to have anything more than a string of triangles forming consecutive quadrilaterals with one diagonal. In two or three cases the configuration of the country was such that the figure became somewhat more complicated, making it possible to compute the length of one of the sides by several routes. This gave in two instances quite discordant results, causing a change in the lengths of the sides from which the succeeding figures derived their values, in one case of 1.1 toises and in the other 0.3 toise. This of itself would cause one to look with considerable doubt

on the reported agreement within two feet between the measured and calculated lengths of the base of verification.

Azimuth observations were made at both bases and at three intermediate stations, using for this purpose the sun. The angle between it and one of the stations was measured with a quadrant. This direction was carried along in the computations and compared with the next azimuth which was determined. The discrepancy always fell below 1′, which implies that no great error could have arisen from orientation.

There yet remained the most difficult and most important part of their labors, the determination of the arc's amplitude. It was decided to observe simultaneously at both extremities of the arc the same star ϵ Orion, and to determine its zenith distance, the difference of these distances giving the amplitude. For these observations they were provided with sectors having a graduated arc of about thirty degrees, and of eight and twelve feet radii; this long arc, however, was removed, and one of four to five degrees substituted in its place. This new limb was divided by laying off on it a chord which was an aliquot part of the radius of the sector, this arc being very near the double zenith distance of the star. From these terminal points

on the limb the distance to the optical axis of the telescope was measured by the micrometer. The meridian was found by allowing a ray of sunlight to enter through a small hole in the roof of the observatory. The line was then marked by the suspension of delicate plumb-bobs, using a hair for the cord. Precaution was taken to see that the limb was kept in line with these hairs.

During the work the instrument was frequently reversed, so as to secure values under different conditions, and the results were found to be so harmonious that no results were discarded because of incongruity. The value of the micrometer was found by measuring with it an angle of known dimension.

The plan of simultaneous observations on the same star was the first step towards eliminating the influences of precession, aberration, and nutation. But the correction for refraction was applied arbitrarily. The results obtained from this set of observations agreed so well with those obtained from individual operations that the final series were made at Tarqui during December, 1739, and January, 1740, and at the northern end of the arc in February, March, and April of the same year.

As was to be expected, Godin decided to determine the amplitude of his arc by himself.

He had a sector of long radius, eighteen or twenty feet, but just what he obtained for the amplitude remained a secret.

Everything seemed to be complete when La Condamine, on May 6, 1740, went to Quito to transact some legal business caused by the death of their surgeon. They had congratulated themselves upon the completion of their labors and the close agreement of their results, and perhaps daily expected further confirmation from Godin. Numerous delays prolonged the stay of La Condamine at Quito; this afforded Bouguer, who appears to have been an indefatigable worker, an opportunity to investigate the effect of flexure in the limb of their sector, the error to be feared from the expansion and contraction of the radius, and the stability of the sector itself. This last subject contained for him surprises. This instrument was made in parts for convenience in packing and transporting, and when it was put together two screws were nowhere to be found. This necessitated a patching together of the parts with wire and cords. Bouguer became fully impressed with the belief that their amplitude was unreliable, so he at once began the construction of a new sector. In 1741, the observations at Tarqui were repeated, and found to be so far at variance with the former series that it was decided to re-observe at both termini.

It was not until towards the end of 1742 that they were enabled to make simultaneous observations at their terminal stations, using two different stars. However, when compared with their first result they gave a mean differing only slightly from their first. The following are the amplitudes adapted from the three stars: —

ϵ Orion	3° 7′ 1″
θ Aquillæ	3 6 59
Aquarius	3 6 58

but the average of *all* the individual results is 3° 6′ 58″.

La Condamine took 3° 7′ 1″ for the amplitude, and announced as the length of a degree at the equator reduced to sea-level 56,749 toises, while Bouguer, taking the mean amplitude just given, and a somewhat different value for the length of the entire arc, gave for this same degree at sea-level 56,753 toises.

The reduction to sea-level contains several possible sources of error. Elevations were determined principally by vertical angles, with the base-line known only from the latitudes of the points and the azimuth of the connecting line. Both of these factors might have been affected by grave errors which would conspire to make the final result wholly unreliable.

Bouguer returned to Paris alone in June,

1744, and was followed eight months later by La Condamine. Scarcely had he set foot on his native soil when he became involved in bitter quarrels with Bouguer, which lasted so long that he finally lamented having participated in the work, saying that ten years of labor in the New World were followed by as many of controversy in the Old.

When the length of a degree of the Peruvian arc was compared with either of the northern arcs, it was seen that the degrees increase towards the pole, and hence the earth is oblate. This outcome may have been the cause of Bouguer's bitterness, because he was known to be a believer in the prolate hypothesis.

In the discussions of this arc which followed the publication of the reports of the participants, but little could be done besides examining the astronomic determinations and base measurements. Delambre has changed Bouguer's result by 16 toises, while Von Zach modifies it by 38 toises. Laplace found that it would be necessary to increase the amplitude by 10″ in order to give a figure to the earth conformable to our best beliefs.

The monuments so carefully erected by La Condamine were very soon afterwards destroyed by the authorities of Quito, otherwise it is likely that the astronomic observations would have

been repeated long ago. Humboldt saw one of the tablets from these monuments in the old Jesuit college at Quito.

Although the task of measuring the length of a degree was the only one imposed upon Bouguer, yet he made good use of his opportunities and made some valuable contributions to other sides of this general problem. It would be out of place to refer to his methods as well as conclusions; but our opinion of his abilities must improve as we learn that he made observations looking towards a determination of the density of the earth's crust; sought to ascertain the amount of attraction exerted by a mountain mass on the plumb-line; and looked for another solution for the figure of the earth deducible from the length of the seconds pendulum. In swinging his pendulum he introduced nothing new, but in his reductions he made allowance for the first time for buoyancy—an element which recent dynamic investigations have emphasized.

Again, the remark may be permitted that this arc of such doubtful value presents the only element so far as the western hemisphere is concerned in the general determination of the shape of the earth. European nations have vied with one another in elaborating data for this grand solution, but the misdirecting spirit of immediate profit and utilitarianism of the people of this

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country have kept our coast and geodetic survey — the recognized advance guard of geodetic science—so incessantly occupied with work which must bring forth fruit in a season that only partial arcs have been measured, and even these on the plea that they were necessary or incidental to the regular work. The new order of things, however, will make it possible to carry forward this important work, and then, when the French government shall have carried out its expressed intention to re-measure this Peruvian arc, — which the present geographical boundaries put in Equador, and not in Peru, — we shall feel that our continent is having some voice in deciding the momentous question as to the exact size and shape of the earth.

CHAPTER VI.

CONTINUATION OF SPASMODIC GEODETIC OPERATIONS.

THE first work of the second half of the eighteenth century was proposed simply to keep a sensitive man from becoming angry. This man was Boscovich. Perhaps at no point in this historic sketch is the temptation to go somewhat beyond the natural limits and give the story of the life of a participant so great as when we fall for the first time on the name Roger Joseph Boscovich. However, there can be mentioned here only the incident which precipitated him into degree-measuring. He had thought of the figure of the earth, had swung pendulums to determine relative gravity, discussed local attractions, and it is believed that while in London he suggested to the Royal Society the value of a degree-measurement in America. While at Rome his versatility caused him to touch many subjects, and his boastful nature and ready speech caused him to announce his theories and conclusions almost before they were authenticated by observation. These, frequently coming into the pos-

session of conscienceless persons, were announced as independent deductions. Such events so wounded the pride of the sensitive Boscovich that he anxiously awaited an opportunity to leave the imperial city. This seemed to come when the government of Portugal made an application to the general of the Jesuits, of which Boscovich was a member, for ten mathematicians to go to Brazil, for the purpose of running its boundary lines. Wishing to unite with that object the mensuration of a degree of latitude, Boscovich's offer to join the expedition was gladly accepted, but Cardinal Valenti, unwilling to lose the companionship of his learned companion, commanded him to remain, and consoled him by promising to secure the permission and means for performing such an undertaking in Italy.

Active operations were begun in the fall of 1750, with the assistance of the English mathematician Mayer, afterwards known as the Jesuit Le Maire. The triangulation was extended from Rome over the Apennines as far north as Rimini, with a base at each end. In general the methods elaborated by the French were employed, but in measuring the bases some new ideas of considerable merit were introduced. They had three wooden bars about three toises long, made from a ship's mast; near each end

of these was attached a plate of brass bearing a very fine mark. The length of the bar was taken as the distance between the marks near the two ends, and in measuring, the bars, resting on tripods, were not brought into actual contact, but simply near enough for the observer to ascertain by means of a pair of dividers the distance between the forward mark of the rear bar, and the rear mark of the forward bar. This method prevented the disturbance which would inevitably arise from bringing one bar directly against another, and was the forerunner of certain forms of apparatus which will be described later. The rods were made horizontal on their tripods, and when the ground was such that this became impossible, the rear end of the forward bar would be brought under the plumb-line let fall from the mark on the rear bar. Unfortunately, the rods were supported at their ends and not at points one fourth the length from each end, so that considerable error is to be feared from what we now call sagging, although this was allowed for, but with doubt as to its accuracy.

Frequent comparisons were made with an iron toise copied from the toise of Peru, using in this connection the coefficient of expansion given by La Condamine, and temperatures obtained by placing a thermometer on the bar during comparison. The length taken for any day was the

mean of the two obtained at the beginning and end of that day.

The base at Rome was along the Via Appia, beginning at the tomb of Metella; and from a single measure its length was taken as 6,139.66 toises; the Rimini base was measured twice with a difference of 2 inches in 6,037.63 toises. The ends of this base were found after an interval of 56 years, when Mognet was making a map of Italy.

The two bases were connected by a chain of 11 triangles, and through the base at Rome the Rimini base was computed, giving a length .97 toise less than was obtained by direct measurement—a result hardly to be expected when it is considered that the angles were not reduced to the same horizon, nor was there any adjustment to geometric conditions. A degree in latitude 43° gave 56,973 toises.

Nicholas de La Caille, of whom mention has already been made, received from his faithful friend and instructor, J. Cassini, the inspiration to contribute to his favorite subject, astronomy. After carrying on for seven years a series of observations at the Mazarine College, he formed a project, which was approved by the French court, of observing the southern stars at the Cape of Good Hope; and accordingly set out in 1750 for his new field of labor. At this

time the Cape was in the possession of the Dutch, but as they were friendly to the French, La Caille, an abbé, as he was usually styled, although he only studied theology without ever becoming an ecclesiastic, found no difficulty in obtaining permission to erect his observatory at the capital of the colony. During his two years' residence there he determined the positions of about 10,000 stars which were visible in the southern hemisphere, in addition to other work, such as the determination of the parallaxes of the sun, moon, and some of the planets, the obliquity of the ecliptic, amount of refraction, and a terrestrial measurement of degrees.

Just coming from the theatre of discussion as to the exact size and shape of the earth, and reared in the family of the Cassinis, it was quite natural that La Caille should seize with avidity upon every possible opportunity to engage in an attempt to assist in finally settling this vexed question. He laid out a chain of triangles, extending from his observatory to Klyp Fonteyn. The angles were measured with a quadrant of three feet radius, read by a micrometer. The base-line had its length determined by using four rods of fir, each 18 feet long, 3 inches wide, and 2 inches thick. The rods were coated with two thicknesses of oil, to keep out moisture, and tipped with iron. He had taken with him a new

toise, adjusted to the Langlois toise, — a go-between between the Peru and Lapland toises. Of this he made an exact copy in wood with copper ends; the measuring rods were then tested by placing them level and end to end, and applying the two standard rods to the entire length. This precaution was taken on each of the mornings during which the measuring lasted — from October 17th to 25th. No discrepancy was detected, at least none was allowed for, nor did he regard temperature as a disturbing or dreaded factor. The first measurement of the base gave 6,467 toises 4 feet 3.5 inches, while the second was exactly 8 inches longer. After applying corrections to both for irregularities in the ground and errors which he deemed sensible, he accepted as the mean of his two measures 6,467.25 toises.

Latitude was determined by zenith distances, using for this purpose a six-foot sector. The amplitude, the difference of latitudes of the terminal stations, was $1° 13' 17\frac{1}{3}''$, which, combined with the terrestrial distance, 69,669.1 toises, between their parallels, gives 57,037 toises as the length of one degree in latitude $33° 18' 30''$ S. This was so much larger than had been accepted as the length of a degree in the same latitude north, that he thought the northern and southern hemispheres were not symmetrical.

However, the short time in which this work was completed — about two months — and the somewhat rude means employed render the result of little value as an element in determining the earth's figure.

Realizing the importance of accurate degree-measurements in southern latitudes, Maclear, while stationed at the Cape in 1848, made, under the auspices of the British Admiralty, an extension of La Caille's arc to the northward. He was unsuccessful in his attempts to re-occupy the latter's stations, so that his labors were rather an extension of La Caille's arc than a verification, as is usually stated. His arc of $3\frac{1}{2}$ degrees rests upon a carefully measured base, while the amplitude resulted from latitudes observed with the famous Bradley zenith sector; and in passing it may be stated that in Maclear's account of operations at the Cape can be found an elaborate description of this instrument, together with many illustrations. Maclear found for a degree in latitude 35° 43′ 20″ S., 56,932.5 toises; a value of 47 toises less than the French found for a degree of the 40th parallel north.

In 1761, Cassini de Thury, sometimes called Cassini III., began an arc of parallel which, when completed, should extend from Brest to Vienna. The crude methods then employed for determining the difference of longitudes made it

impossible to know the amplitude of an arc of parallel with anything like accuracy. This work was evidently projected with the very commendable purpose of affording a continuous chain of triangles to serve as the skeleton of maps of contiguous countries, thus making these maps harmonious when made by the various governments, and at different times. Still he could not go counter to the hereditary fondness for the problem of determining the shape of the earth, so when he found himself, two years later, in the possession of geodetic data, he published his length of a degree.

The immediate outcome of this work was the interest which it awakened in Germany and Austria in geodetic operations. The base in this chain at Mannheim was measured, with the assistance of C. Mayer, who, in his ignorance of what others had done, boasted of improvements long since adopted. In Austria, the triangulation rested upon a base measured by Liesganig, near Vienna, in 1763. The section in Bavaria rested upon the Munich base, but it was so carelessly marked that when, in 1764, the Bavarian government decided to extend, for cartographic purposes, a net over the kingdom, Osterwald found it necessary to measure a new base. In prosecuting this work a unique feature was introduced. He drove stakes in the ground at

suitable distances apart, and nailed on the tops boards so as to form a bridge on which to lay the five bars with which the measurement was made. The rods were placed end to end on this tressle, and when the fifth one was put in position it was made fast to its bed by means of a screw, and remained so until the other bars were brought forward one by one and put in place. This plan has not been deemed worthy of copying in the recent methods. Temperatures were recorded, and when the second measurement, executed at a higher temperature, gave a greater length, he naïvely jumped at the conclusion that wood, of which the bars were made, contracts with rising temperature and expands with cold. This is an early instance to attempt to formulate a theory from inaccurate data, preferring to contradict accepted beliefs than to acknowledge errors of observation.

Two names are now reached which have for the people of this country considerable interest; they are Mason and Dixon. The mention of these names suggests to the minds of our people two distinct ideas, united in many cases with a commingling that strangely represents tradition tinged with truth. The clearly defined notions are: 1. Mason and Dixon determined the length of a degree in America by measurement. 2. They located the boundary between Maryland on the

one part and Delaware and Pennsylvania on the other. These are facts, but popular belief adds one or both of the following fancies: *a*. They measured the arc from which they ascertained the degree's length while fixing the boundary. *b*. The boundary they established between Maryland and Pennsylvania was the northern limit of slavery, established by the Missouri compromise of 1820. The first misconception will be rectified in what follows, in which it will be shown that Mason and Dixon did not mark that portion of the boundary which they measured with sufficient care to use its length for geodetic data; and the latter will be refuted when it is stated that the line agreed to in the compromise was latitude 36° 30′, while the boundary line referred to is latitude 39° 43′.

According to the agreement made in 1732, regarding the division between Maryland and Delaware, a line was to be run across the peninsula from Cape Henlopen due east and west, then from the middle point of this line another should be run northward in such a manner that it should be tangent to a circle drawn around New Castle, with a radius of twelve miles. From this tangent point the line was to be continued due north to a point whose latitude was that of an imaginary point just fifteen miles south of the most southern point in Philadelphia. Lawsuits

and other sources of delay followed, so that it was not until 1760 that commissioners were appointed, who ran the line across the peninsula, determined its middle, computed the position of the point of tangency and ran the tangent. As the entire line was through dense forests, it was necessary to cut out sight lines or vistas, making the work so tedious that three years were spent in performing this part of the work. The proprietors, who resided in London, became so impatient that they decided to send surveyors from England to complete the undertaking, and to verify what had been done. The persons selected were Charles Mason and Jeremiah Dixon.

The former was an assistant of Bradley at Greenwich, and one would infer that he was an accomplished astronomer from the fact that he published a revised edition of Mayer's "Lunar Tables," which had received the prize offered by the board of longitude for the best means of determining longitude at sea. The latter was likewise an astronomer, judging from the fact that he was a member of the party sent to the Cape of Good Hope in 1761 to observe the transit of Venus.

They arrived in Philadelphia November 15, 1763, and began work by erecting a small observatory at the most southern part of the city. Here they observed for latitude, using a sector

of six feet radius, which Maskelyne said was the first to have the plumb-line passing over and bisecting a point at the centre of the instrument. During the following spring, while testing the lines run by their predecessors, they noticed that the tangent line was almost due north, and the character of the ground such as to make it quite easy to measure the line with the accuracy that would allow the results to be used in ascertaining the length of a degree. Also, that the amplitude could be found by simply observing for latitude at one additional point, having already determined the latitude of the northern end of the arc in fixing the point from which the west line between Pennsylvania and Maryland was to start.

It is possible that Mason and Dixon received their inspiration to perform this geodetic work from Boscovich, whom they may have known during his stay in London in 1760. This is quite likely, as it has already been stated that he referred to the desirability of having an arc in America. Boscovich had been elected a Fellow of the Royal Society, in consideration of his degree-measurement in Italy, and Mason and Dixon, while in the swamps of the peninsula, might have seen along the vista in which they were working the coveted way to become, as De Morgan said, a "Fellow Really Scientific;" at

all events each became a F. R. S., in consequence of this very undertaking.

They represented to Maskelyne, who was then Astronomer Royal, the advantages this line offered for accurate measurement, and indicated their willingness to take charge of the work at the society's expense. On October 4, 1764, the Council of the Royal Society resolved that the precise measure of a degree of latitude in America, in the neighborhood of Pennsylvania, appears to the Council and to the Astronomer Royal, who was pleased to assist on this occasion, to be a work of great importance, and that the known abilities of Messrs. Mason and Dixon, the excellence of the instruments with which they are furnished, the favorable level of the country, and their assistants well practiced in measuring, do all concur in giving good ground for hope that the business may now be executed with greater precision than has ever yet been done, and at a much less charge than the society can reasonably expect an opportunity of doing hereafter.

"*Resolved*, to employ Messrs. Mason and Dixon in the said admeasurement of a degree of latitude, and to allow them the whole of their demand, being the sum of two hundred pounds sterling for the said work; and also, in case the proprietors of Maryland and Pennsylvania

should refuse their stipulated allowance for their passage home, but not otherwise, the further sum of forty pounds for the said passage." A resolution was adopted requesting Maskelyne to draw up the instructions for their guidance. These were sent, together with a brass standard of five feet, with which the measuring rods were frequently to be compared, and the difference noted, and also the height of the thermometer at the time.

Work on the Pennsylvania-Maryland boundary was stopped fifteen miles before the terminus was reached, by the Indians, who said it was not the wish of the chiefs that a certain war-path should be crossed, so Mason and Dixon returned to Philadelphia, and at once prepared to make the measurement so encouragingly indorsed.

While testing the line they intended to measure it was found that the alignment was practically perfect, leaving nothing to be done but to determine its length and amplitude. The apparatus they devised for measuring they called levels, because they were placed in a level position when in use. As the ground was not perfectly horizontal, the apparatus was so constructed as to make contact on different planes. The levels were each 20 feet in length and 4 feet in height, made in a rectangular form of

inch pine. The breadth of the bottom board was $7\frac{1}{2}$ inches, that of the top 3 inches, and the end pieces $4\frac{1}{2}$ inches, while the bottoms and tops were firmly strengthened by means of boards fixed to them at right angles. The joints were secured by plates of iron, and the ends were plated with brass. They were set level by means of a plumb-line suspended in the middle, the point of the bob bisecting a given mark in the bottom ; the line was hung within a tube, so as to be free from the action of the wind. If the frames were true rectangles every point in the end would be in the same vertical when the top and bottom were horizontal. In case the ground was irregular one end of the level was raised, and when in position contact was made, then the rear frame was brought forward and adjusted. Alignment was effected by sighting along the top to the point made by the apparent convergence of the sides of the vista.

Record was kept by stretching, in a line parallel to and quite near the levels, a rope equal to twelve times the length of one of them, and the number of rope lengths recorded. Streams were crossed by measuring a short base and computing the distance from a point on one bank to another on the bank opposite, from angles observed with a quadrant.

Frequent comparisons were made with the five-

foot brass bar, and all were reduced to the temperature of 62° F., using Graham's coefficient of expansion for brass. The direction of the line was found from observations on the pole star.

The result of their measurements, when projected upon a meridian, gave 538,067.2 feet for the length of the arc whose amplitude, obtained from zenith distance differences, was 1° 28′ 45″; this gave for one degree in latitude 39° 11′ 56″, 363,763 feet.

It was assumed that the French foot was to the English foot as 114 to 107, which gave for a degree 56,904.5 toises; but a subsequent comparison of the brass rod with the *toise du Pérou*, changed the above value to 56,888 toises, which is the one most frequently quoted.

The actual value of this work has received various estimates: Maskelyne considered it a valuable addition to the measures of degrees, especially as the level character of the country to the north and south of the line rendered a deflection of the plumb-line improbable. Cavendish, on the other hand, suggests that the Alleghany Mountains may have shortened the degree by 60 to 100 toises. Airy thought it accurate enough to be used in determining the figure of the earth, as did also Schubert, Listing and Laplace. Bessel and Clarke did not embody this arc in their discussions, hence its

influence is not felt in their resulting constants. Judging from the apparent adaptability of Clarke's formulas to the United States, as revealed in the experience of the Coast and Geodetic Survey, Mason and Dixon's labors were in vain, as their degree is 73 toises shorter than Clarke's spheroid would give for the same latitude.

As has been stated, Boscovich was interested in the attraction exercised by mountains, and realizing that the geographical position of a point as obtained by computation through a chain of triangles might be compared to advantage with the results of direct observation, he induced Beccaria in 1768 to carry a triangulation from the plains of Turin towards the Alps and there see how the computed latitude agreed with the observed.

In this connection it may be well to call attention to the fact that if a plumb-line is allowed to hang freely, totally unaffected by disturbing forces, its direction is such that if it were extended upward it would pass through the zenith, and the angular distance from this point of intersection to the celestial equator, which is simply the extension of the terrestrial equator, is the latitude of the place at which the plumb-line was suspended. All latitude determinations depend upon this principle, so that if anything

deflects the plumb-line an erroneous latitude will result. If now the latitude is known through computation, the difference between it and the erroneous latitude will be the effect of the disturbance and is a measure of it. It was to determine this, or the station error as it is now called, that Beccaria undertook his triangulation.

In prosecuting this work the only special advance that was made was in the point of base apparatus employed. In shape it had a cross-section like an inverted T, the flat part resting upon tripods. This was somewhat like a forerunner of the most recent form; as in the Brunner apparatus, the bars lie on a double T-truss. At both ends of the two bars fine lines were drawn on the upper surface, so that the measuring length was the distance between the two marks at either end, and while measuring, the mark on the forward end of the rear bar was made to form a continuous line with the mark on the rear end of the forward bar. This was an improvement on the contact forms. The tripods admitted of a vertical and horizontal motion; these were placed under the ends of the bars, which permitted a sagging. This was determined and allowed for by keeping a silk thread stretched taut along the top, fastened at the ends only, then measuring the space between this and the bar by means of a graduated wedge.

The work was not carried forward with sufficient care to be of any value in determining the amount of local attraction, but desiring that his name might be associated with geodetic labors, he announced that in latitude 44° 44′ a degree had the length of 57,024 toises.

Cassini de Thury was not satisfied at awakening an interest in geodesy in Germany and Austria alone; he wished to arouse at least some of the Italian states to unite with the chain which he was stretching eastward. And though, as already intimated, his work in itself had no value from a geodetic standpoint, nor was his reputation enhanced by it, yet it was of great value as an incentive. It was the direct cause of the work in Lombardy, which, though executed for purely cartographic purposes, contained some new features of great importance.

The most valuable improvements were made under the direction of the astronomers of the observatory of Milan, and were concerning base apparatus and base measurement. They employed a bar of iron, T-shaped, with the lower flange resting in a groove in a bar of wood rectangular in shape. The wooden bar was supported on tripods whose heads were provided with a vertical motion by means of a screw, and a slight horizontal motion, with set-screws to hold the bar in place. Contact was made as in the

bars of Beccaria, and was greatly facilitated by having the bars sliding within their wooden grooves on rollers attached to the latter. For the first time we find used an instrument for determining the angle of elevation or depression. It consisted of a flat piece of metal; to its upper face was hinged a level which could be brought into horizontality by means of a screw passing through the other end of the frame of the level, and impinging on the metallic bed. The head of the screw carried a pointer, which, sweeping over a small dial, showed how many turns or parts of a turn of the screw were necessary to bring the bubble of the level to the centre. This instrument was placed on the top of each bar when in position and made level; then, knowing the value of each turn of the screw in arc-measure, it was easy to tell the inclination of the bar, and hence the correction which should be applied to reduce the bar to its horizontal projection. The bars were supported on the tripods at points one fourth of the length of the bar distant from each end. Another great improvement was in the alignment. For this a telescope was placed successively at both ends of the base, and stakes adjusted in the line of the optical axis when the other end was sighted. With a number of stakes thus placed in line, the bars were aligned by sighting along their tops to two or more of these stakes.

No degree value was deduced from this work, but as the two measurements in 1788 of the Somma base gave a discrepancy amounting to one unit in five hundred thousand (1: 500,000), one must feel sure that their work was well done.

There may be mentioned in this chapter, because of the character of their work rather than for chronologic sequence, the following values for a degree: —

Liesganig,	1762–1769,	57,086 toises	in latitude 48° 43′
Barrow .	1790 . .	56,725 toises	in latitude 23° 18′
Nouet .	1798 . .	56,880 toises	in latitude 27° 39′

CHAPTER VII.

GEODETIC WORK IN ENGLAND.

During the progress of the rebellion which broke out in the Highlands of Scotland in 1745, it became apparent that there was an imperative necessity of having that country, so inaccessible by nature, explored by placing military posts in its inmost recesses, and uniting them with one another and the outer world by roads. With this in view, Fort Augustus was established in 1747. Attached to this camp there were two indefatigable officers, — Watson and Roy, — who, not satisfied with the meagre energies called into play by camp routine, began to map the surrounding country. This project, first intended for the Highlands, gradually extended over a part of the Lowlands, but was stopped by the war of 1755.

On the conclusion of the peace of 1763, the general government took under consideration a survey of the entire island, but the usual delays which attend work that is to be done at public cost prevailed, and nothing was accomplished

before the American war demanded England's complete attention. However, when quiet was restored in 1783, the desire to have a good map was again felt, and Roy, remembering his experience in the mountains of Scotland, had been devising plans for prosecuting the work, when ordered by the government, in the best possible manner. He had even gone so far as to measure a base, and surround a part of London with a network of triangles. But before his labors had brought forth any tangible fruit, he found that more stupendous operations were in view.

In October of that year, the French ambassador laid before the Secretary of State of England a memoir by Cassini de Thury, in which was set forth the advantage that would result if the observatories of Greenwich and Paris were connected by triangulation, as at that time their relative positions were not known with any certainty. This proposition was referred to Sir Joseph Banks, President of the Royal Society, who placed General Roy in charge, the government granting the necessary funds.

The base line for England's share of this work was measured in Middlesex, and is known as the Hounslow Heath base. Considerable attention was given to the form of apparatus and the material in its construction; finally it was decided to use rods made of Riga wood. They

were tipped with bell-metal for end contact, and were provided with plates of ivory on the upper surface near each end, on which were drawn fine lines for coincidence in measuring, both plans having their advocates. Roy very wisely preferred the latter plan, saying that with it errors of coincidence were as likely to be in one direction as the other, and hence were counteracting, while in the former plan, the jar of contact was always backward, and hence the errors were accumulative. This is one of the first attempts to formulate any theory as to the compensating character of certain errors. As an end measure the bars were 3 inches in excess of 20 feet. The bars were trussed so as to insure rigidity, but as they were supported at the ends there is no guarantee that sagging was not present.

It was about this time that Ramsden was making such valuable experiments in the matter of comparison of standards of length, resulting primarily in a beam compass comparator provided with a micrometer screw capable of measuring to the $\frac{1}{5000}$th of an inch. With this instrument a length of 20 feet was laid off, and a beam compass was made of wood and adjusted to this length, serving thereafter as the standard with which the rods were compared.

The line was divided into sections of 600 feet; at each end tripods of the same height were

placed; on one of these a telescope was set and directed towards the other. Then, when the bars were placed in position, they were put in the axis of this telescope both laterally and horizontally, thus measuring an hypothenuse of 600 feet. The difference in the height of these terminal tripods was ascertained by means of a spirit-level, which, giving two sides of the triangle, made it possible to compute the third side or the projection of the hypothenuse.

When it was necessary to stop for the day, a plumb-line was let fall from a fixed mark on the rod, and the exact position of this line was fixed by moving in the direction of the base a slide which was attached to a frame firmly fastened to the ground. This slide had on its upper surface a fine mark, which, after being brought into coincidence with the plumb-line, served as the starting point when work was resumed. In order to secure the line from disturbance by the air while in suspension, the bob was immersed in a cup of water.

After the measurements had begun, repeated comparisons at different temperatures showed that a change of a few degrees made but little difference in the length of the bars, but as soon as there was a marked change in the humidity of the air, it was noticed that the bars differed materially from the standard. This

suggested to General Roy that some metal be employed in place of wood, and in a short time he concluded to try cast-iron, as it had a smaller coefficient of expansion than had steel. Colonel Calderwood, however, proposed that glass be tried, and as tubes were more easily made than rods, it was decided to make the experiment with tubes. While these tubes were in process of manufacture, the entire base was measured with the wooden bars.

As several new features, afterwards employed, were introduced in this apparatus, it may be well to describe them here. The glass rods were inclosed throughout the greater part of their length in a wooden box, leaving both ends of each rod projecting. Sliding within the stopper which closed one of the ends of each tube there was a pin held out by means of a spiral spring; the outer end of this pin terminated in a rounded head, while on this pin cross-lines were drawn and on the glass outside there was a fixed line. When this pin was thrust in until the centre of its cross-lines was directly under the line on the tube, its outer end was just 20 feet from the fixed terminus of the other end of the tube. This has been since named the slide-contact. The boxes were drawn together, after being placed in line and in the same plane, by means of a screw fastened to one box near the end, and

bearing against chucks on the top of the other box. Each box was provided with two thermometers lying on the upper surface, but with their tubes bent at right angles so that the bulb was within the box.

It was decided to discard the result of the measurement executed with the wooden rods, and to go over the base with the glass rods and a steel tape — the latter being supported in several places and kept taut under the constant tension of 14 pounds. The two measurements agreed so well that it was apparent that no great error had been committed, and the length was announced to be 2,704.0137 feet at a temperature of 62° F., reduced to the level of the sea.

It was not until 1787 that a concerted action was taken to join England and France. In the interim, however, Ramsden was at work upon an instrument of his own invention for measuring horizontal angles, known since then as the theodolite. He had in 1763 invented a graduating machine or engine for dividing automatically rulers or linear measures, so when it was proposed by Tobias Mayer in 1770 that entire circles instead of sectors be used on angle-reading instruments, Ramsden was not slow in adapting his engine to circle dividing. He went still farther, and added reading microscopes to his

theodolites instead of the line which Mayer used. As this is the first time that an entire circle was used, it might be well to explain its advantages: First of all, in its graduation the entire 360 degrees had to find a place; therefore, if on some parts of the circle some of the divisions were too large there must be other parts on which they were correspondingly too small: hence, if all parts of the circle were used in measuring an angle, the average value could not be far wrong. Borda had made a repeating circle in 1785, but it was not comparable to Ramsden's theodolite: it was simply the entire circle provided with two telescopes, one attached to the circle, the other moving on the same pivotal point, but free from the circle. Both were pointed at the initial point; the second was clamped in that position, and the first one was turned until the second point was bisected; as the zero point was stationary under the second telescope, and the circle moved with the other one, the reading of the circle gave the angle. No. 2 was now unclamped, pointed at the second station and clamped; both were moved back — circle and all — until they pointed at the first station, then No. 1 was turned to the second station. The angle was now half the reading of the circle-reading; this process might be continued to as many repetitions as were deemed necessary, when the final

reading — counting the number of times the entire circle was passed over — was divided by the number of repetitions. This plan is still used by some who act upon the fallacious belief that the only error made was in reading the circle, and therefore, if this was done only once, it would be divided by the number of pointings, and hence could be diminished at will by increasing the repetitions. However, in the modern instruments, the telescope and circle can be made to move together, or the former move while the latter remains stationary, thus using only one circle.

Ramsden's theodolite had a circle three feet in diameter, divided into ten-minute spaces, and read by two micrometer microscopes. One turn of the micrometer-screw was equal to one minute, and as the head of the screw was divided into sixty parts an angle could be read to a single second. The success attained in the use of this instrument, giving a maximum error of closure of three seconds, was regarded as truly phenomenal. The telescope attached had a focal length of 36 inches. This theodolite has had a varied experience at home and in India, and was, according to Colonel Clarke in 1880, as good as when it left the workshop.

The English Channel was crossed by sight-lines in the neighborhood of Dover, the English

occupying two stations on their side, while the French, Cassini, Legendre, and Mechain, occupied simultaneously two stations in France — they using a Borda circle. The signals were white lights, and although bad weather prevailed the angles were read with due care. General Roy thought it best to measure near this junction a base of verification, which was accordingly done, using the steel tape ; the French did not measure a new base, but let their triangulation rest upon their former work.

In the reduction of his field work, Roy made due allowance for spherical excess. As geodetic triangles are on the surface of a figure more or less spherical, the sum of the angles in each will exceed 180°, the limit of the angles of a plane triangle. This excess is a function of the radius of the spheroid and the size of the triangle. Legendre had just shown that when the area of a triangle is very small compared with the entire surface of the sphere, it is sensibly equal to a plane triangle whose sides are respectively equal in length to those of the spherical triangles, and whose angles are equal respectively to those of the spherical triangle, each diminished by one third of the spherical excess.

The attempt to deduce the length of a degree from this work was not at all satisfactory, perhaps owing to the different methods of determining the latitudes of the terminal points.

It was evident that a map in order to be reliable must rest on a good scheme of triangles, and as England now had made a beginning it was decided to push the work ahead. This, however, was not definitely decided upon until 1791, when the Ordnance Survey was placed under the direction of Colonel Williams, Captain Mudge, and Isaac Dalby. Their first labor was the re-measurement of the Hounslow Heath base, using a steel tape. Their reason for doing this work over was the belief that every base should be measured twice; however, they obtained a result differing by only three inches from what General Roy found. In this work a transit instrument made its appearance for the first time, it being used to direct the aligning of the base.

It was decided to replace the wooden monuments which marked the termini of the base with cannon. In the task of placing a mark on the cannon at the exact end of the base a plan was devised which is still in use. Before removing the original mark, four stakes were driven each about ten feet from the mark into the ground, two of them being in the direction of the base and the other two at right angles to it. Silver wires were then stretched from the tops of the opposite stakes, and moved until their intersections coincided with the mark on

the monument; and in this position a fine line was drawn on the top of each stake directly under the wire. The monument was taken up, the cannon put in place, the wires drawn and their intersection marked thereon.

Up to 1801 the main object of the Ordnance Survey was to form a skeleton of an accurate map, and therefore no accuracy was sought beyond what was essential to this object. Numerous bases of verification had been measured, revealing discrepancies between the measured and computed values of from one to five inches. But as only one measurement of each base was made, it was difficult to know wherein lay the source of error. However, as the progress of the work carried the triangulation from the most southern point of England far to the northward, it is natural that the idea of degree-measurement should suggest itself. As the only additional factor needed was carefully determined latitudes, an elaborate zenith sector was made and at once employed at the terminal stations, and at a point approximately midway along the arc. The amplitude of the entire arc was found to be $2° \, 50' \, 23''.38$, and the length of a degree in latitude $52° \, 2'$, 60,820 fathoms (57,109 toises), in latitude $51° \, 35'$, 60,864 fathoms (57,068 toises). This was somewhat damaging to the oblate hypothesis, as these degrees decreased

towards the pole. General Mudge did not doubt the accuracy of his terrestrial work, but thought the error was caused by unequal deflections of the plumb-line at Dunnose, with water to the south, and Clifton, where the water was to the north, — in both cases in the opposite direction from the arc. But when Colonel James a few years later revised the entire chain of triangles, an error of 170 feet was detected in the length of this arc.

The publication of the results above named, and the observations from which they were deduced, called forth heated discussions from those who doubted the accuracy of the methods employed, and those who had faith in the participants in the work. It was sufficient to impress upon the people the necessity of revising the entire work to see if the country which gave birth to the oblate hypothesis really negatived its existence.

By this time the theory of *least squares* was carried far enough along to reveal the possibilities of its application to triangle adjustment. The principle which lies at the bottom of this theory and gives it its name is this: from a number of values for the same quantity some one must be chosen; this one will differ from each of the others by a small amount called a residual: now that value is deemed the best

which makes the sum of the squares of the residuals a minimum. The application of this theory to triangulation is especially along the adjustment of the angles. Thus the sum of the three angles of a triangle after the correction for spherical excess should equal 180°, but they seldom do in practice, which gives a residual; again, when two angles are measured individually and as a single angle, the sum of the two individual angles should equal the whole, which will give residuals; also when all the angles at one point are read, closing the horizon, their sum should equal 360°, giving other residuals. These and other conditions, varying with the character of the work, can be expressed in the form of equations which require in their solution that the sum of the squares of the residuals be a minimum. When the equations are solved and the values corrected, all the conditions will be fulfilled.

With this control over the computations, it was necessary that the observations should be good, and that the triangulation should rest upon bases measured with the greatest possible care. Realizing that the uncertainty in measuring a base arose chiefly from the coefficient of expansion which was employed, and the ignorance of the exact temperature of the measuring unit at the time of its use, General Colby de-

vised an apparatus which theoretically should have the same length at all temperatures. It was called the compensating apparatus, and consisted of a bar of brass and a bar of iron, fastened at their centres, but free to move the rest of their lengths. Each end of one of the bars is a fulcrum of a transverse lever attached to the same end of the other bar, the lever arms being proportional to the rates of expansion of the bars. In this way the microscopic dots on the free ends of the levers are theoretically at the same distance apart for all temperatures. As the terminal points were the dots on the lever arms, contact could not be made in measuring, but one of two fixed microscopes attached to a separate stand was adjusted over the dot on the rear bar, and the dot on the forward bar was brought under the other microscope. The distance between the microscopes was added to the length of the bar. In all forms of compensating bars, the components having different rates of heating and cooling, their cross-sections should be inversely proportional to their specific heats, and should be so varnished as to secure equal radiation and absorption of heat.

With this apparatus two bases were measured, the Lough Foyle in 1827–28, and the Salisbury in 1848; the difference between the measured value of the latter and its value as computed

from the former was 4.6 inches. When the other bases were brought into the triangulation their discrepancies averaged 1.21 inches per mile.

The results of this triangulation, extending from Dunnose to Saxavord in the Shetland Islands and having an amplitude of 10° 12′ 31″, were published in 1858. There was likewise another meridional arc extending from St. Agnes Lighthouse on Scilly Island to North Rona, with an amplitude of 9° 13′ 41″. The former arc has been extended through France and Spain.

This closes the active interest or participation of England in geodetic work at home; the years subsequent to the date just given have been devoted by the Ordnance Survey to cartographic work, comparison of standards, and gravity determinations. However, most valuable and important work has been pushed ahead in India by the home government, which will form the subject of another chapter.

CHAPTER VIII.

SYSTEMATIC WORK IN FRANCE.

PICARD, after his successful arc-measurement, devoted considerable time to the determination of the length of the pendulum beating seconds, and had visited several places in France for this purpose. One of these was Lyons, where he met Mouton, who had, as early as 1665, proposed a universal measure, and showed how it could be kept in the length of a pendulum. So when Picard returned to Paris he was full of the idea of a universal unit of measure, and when it was proposed, in 1790, the time was ripe for a change. The revolutionary spirit was so rife that there were no sentimental associations connected with old things to interfere with the introduction of new ones. M. de Talleyrand, the minister of foreign affairs of the new régime, was ready to propose to the constituent Assembly of France that England be invited to coöperate with France in fixing a standard of measure. The decree was rendered on the 8th of August, 1790, and sanctioned two weeks later. According to this decree an equal number of commissioners

from the Academy of Sciences and the Royal Society of London should unite in order to determine the length of the pendulum which vibrates seconds in the latitude of 45° (as proposed originally by Huygens) or in any other latitude that might be thought preferable, and to deduce therefrom an invariable standard of measure and weight.

The Royal Society did not clamor for new things, and consequently did not advise a change, so that the Academy's commissioners were alone in their counsels. They were Borda, Lagrange, Laplace, Monge, and Condorcet, perhaps the strongest committee ever appointed. They took under consideration three different units: the length of the pendulum on the 45th parallel, a definite part of a quadrant of the equator, and a portion of the quadrant of a meridian. The two first possessed serious disadvantages, but supposing the earth to be a figure of revolution, all meridians would be the same, therefore a quadrant of a meridian would everywhere have the same length. Besides this advantage, a meridional arc of considerable extent had already been determined. This caused the commission to decide to take as the unit of length the ten-millionth part of the distance from the pole to the equator, this distance to be ascertained from the length of a degree in latitude 45°. They also

resolved that the arc from which this degree was to be taken should extend from Dunkirk to Barcelona, having an amplitude of about $9\frac{1}{2}$ degrees, and appointed Mechain, who had labored on this arc with Cassini, and Delambre to revise the work that had been done, repeating whatever was necessary, and to extend the arc to Barcelona.

It was not until 1792 that active operations were begun, Delambre having charge of the northern end and Mechain the southern end, Rodez forming the dividing point. It was soon perceived that it was necessary to repeat all of the observations, and at several places new signals had to be substituted.

In this work several improvements were introduced, especially in the base apparatus. Borda constructed a measuring bar of two components, platinum and copper; these rested on a piece of wood, sufficiently strong to be secure from warping and bending, and was covered, at a distance of three inches, with a light case of wood, at the two extremities of which arose two points made of iron to serve as marks to direct the sight in aligning. The two bars were firmly attached to one another at one end, but free to move throughout their entire length, so that the effect of expansion might be carried to the other end, where the copper bar stopped six inches short of the

end of the platinum bar. The bars when completed were placed in melting ice, and when they acquired the temperature of the ice, 32°F., the extremity of the copper bar was marked on the platinum. This was repeated at different temperatures, so that the marks or scale showed the temperature of the bar, the scale readings being directly convertible into degrees. The exact length of the platinum bar was ascertained at a known temperature, and knowing its rate of expansion in terms of the scale, its length could be computed for any scale-reading. In measuring, the bars were not brought into direct contact, but the slight interval was measured by means of a scale sliding out to come into contact with the other bar, this scale being read by a reading microscope. The length of this scale was added to the known length of the bar; the sum of all these lengths reduced to the standard temperature gave the length of the base. This principle is now known as the Borda metallic thermometer, and is employed in several forms of apparatus.

The base of Melun was measured once with this apparatus in forty days; a part of this time was spent in preparing the line and erecting the terminal points. The reduced length was 6,075.7847 toises.

In the angle-reading they used a Borda repeating circle, nor did the French give up this

system of repetition until it had disfigured the observations of half a century.

The entire work occupied the good part of nine years, and they were years of hard toil, accompanied by many discouraging experiences. The revolutionary and unsettled condition of the country made it difficult to pass in safety from one station to another, and their night signals were destroyed because of their suspicious character. The people were ignorant of the nature or purpose of the work, and several times before permission could be obtained to use a church steeple as a signal Delambre found it necessary to give a popular lecture on geodesy, so as to instruct the natives into a sympathetic appreciation of his observations. And before the completion of their labors the entire commission were discharged, because they had not been sufficiently demonstrative of their animosity towards kings. However, Delambre and some others, who were regarded as indispensable, were allowed to bring their operations to a close.

The commission appointed to examine the entire work and to deduce the length of the metre, after having verified all the calculations, determined the length of the quadrant from the data of this new French arc, combined with the arc in Peru. From the French arc, comprised between the parallels of Dunkirk and Montjouy,

with an amplitude of 9° 40′ 25″, and a mean latitude of 46° 11′ 58″, they obtained a length of 551,584.7 toises. The length of one degree on the 45th parallel they found to be 57,027 toises, and the quadrant was 5,130,740 toises.

Mechain had proposed to extend this arc to the Balearic Isles, but his death caused a postponement. In 1806 Biot and Arago were put in charge, and by using powerful lights they were enabled to connect Formentera with stations on the coast of Spain. Within the last few years this chain has been carried through Spain and across to Africa.

In 1841 Puissant discovered an error in the distance from Formentera to Montjouy, which increased the entire length by 68 toises. And a further revision was commenced in 1870, at the base of Perpignam, and has been completed as far as the base of Melun. A few only of the old stations have been refound, so that this work is entirely new. Church towers are no longer used, but when necessary, high scaffoldings are built.

The legal length of the metre is 39.370432 inches, while the length as obtained by Colonel Clarke from more recent arc-measurements is 39.377786 inches. Whatever its real length is, the idea of having a standard recoverable only from such extensive and expensive operations as

the measurement of a quadrant, is purely chimerical. The legal metre is now the length of a *certain bar* at a given temperature, and in obtaining new standards from this bar and in using the metric system, we lose sight of the ratio which it is supposed to bear to the earth's quadrant. It should be mentioned here that when this ratio was proposed, it was expected that the quadrant of circular measure would contain 100 degrees, and that its subdivision be along a decimal scale. This was adopted in France, but has by no means become universal.

After the completion of the great arc between Dunkirk and Formentera, the French government ordered a new triangulation in a direction perpendicular to a meridian which should unite this work with what was already done in Switzerland and Italy, and serve as the groundwork for an arc stretching from the Atlantic Ocean to the Adriatic Sea.

In 1811 Colonel Brousseaud took charge of this work and pushed it along, inducing the Sardinian government, and afterwards the Austrian, to coöperate. In 1821 a commission was formed, consisting of two Italians, Carlini and Plana, and Brousseaud and Nicollet on the part of France. A portion of this work rested upon the Brest base, measured in 1823 by General Bonne. This base was remarkable in that its

length obtained from measurement, using the Borda apparatus, exactly coincided with the length obtained by computation from the base at Melun.

The observatory of Geneva was in this chain, hence all the longitudes were referred to it. Differences of longitude were determined by powder signals at six stations. The results from this work are of no value, owing to the crude means employed in determining differences of longitude. In one case the discrepancy at one of the stations was 31″, while errors of azimuth sometimes reached 50″. Dividing this arc into three parts, the lengths of a degree in each differ by as much as 93 metres, which, if the work were accurate, would show that the parallels of latitude are not circles, and hence the earth is not a figure of revolution.

Since that time the trigonometric work in France, carried on under the direction of the Dépôt de la Guerre, has been solely in the interest of map-making, and has in no way contributed towards the data required in determining the figure of the earth.

When the ellipticities resulting from the three arcs measured under the auspices of the French were compared, serious disagreements were revealed, as follows: —

From Lapland and French arcs, 1:145.
From Lapland and Peruvian arcs, 1:310.
From French and Peruvian arcs, 1:334.

This suggested that the Lapland work, which was measured with great rapidity, and short at best, was the least reliable. Napoleon, at the suggestion of the National Institute, wrote a letter personally to the king of Sweden, requesting permission for some members of that body to proceed to Lapland, in order to determine an arc of the meridian. That high-spirited young monarch replied, that he would consult his own Academy of Sciences at Stockholm, whether such an operation was desirable for the interests of of science; and if they were of that opinion, he had no doubt he could find Swedish mathematicians competent to undertake the task. This explains why the French never verified their work in the north, and their troubles with Spain prevented a revisit to their fields of operation in Peru. However, within the past year, some steps have been taken looking towards a remeasurement and extension of the equatorial arc under French auspices.

Whatever may be done by France in the future in the interests of geodetic science, the nations of the world will always be indebted to her for the impetus she gave to that science while in its infancy, promoting its growth at a time when assistance was most needed.

CHAPTER IX.

SYSTEMATIC WORK IN RUSSIA.

FROM the time of the unification of the several Muscovite states, there was felt the need of statistics and descriptions of its separate parts. The first general map of European Russia was the so-called "Great Plan," compiled in the middle of the sixteenth century. But it was based on very inexact and incomplete data. Systematic operations began in the reign of Peter the Great, who sent out foreigners, especially invited for this purpose, together with the Russians who had been under their instruction, to make surveys of different portions of the empire. These surveys were made in various districts without any general connection; the lines were measured with cords, astrolabes used for measuring angles, and latitudes observed with large quadrants.

With the arrival of the French astronomer Delisle at St. Petersburg (invited by Peter) in 1726, these operations increased, and the Academy of Sciences, founded that year, sent out

special astronomic expeditions, who determined not only latitudes, but also longitudes, from eclipses of Jupiter's satellites, which made it possible to unite the new survey with its antecedents into a geographical map. The result of these expeditions was the Russian Atlas, published in 1745 under the direction of the Academy, and though deficient in many details, this atlas was far in advance of its time, and antedates all general maps except those of France and Italy.

These scientific expeditions continued; Delisle himself went in 1740 to Siberia to observe the transit of Mercury, but failing in his chief object on account of cloudy weather, he made a number of geographic determinations in order not to come back empty-handed. The occasions of the transit of Venus in 1761 and 1769 took astronomers into different parts of the Empire, and each one determined the position of many places, so that by 1789 the positions of 62 stations in Russia were known.

In the eighteenth century an arc-measurement was planned, nor is it known at the present time why the work was not carried out. Delisle thought it possible to have an arc of 22° or 23° in the latitude of St. Petersburg, and in the year 1737 a base-line was measured on the ice between this city and Cronstadt, and several stations were selected.

In 1796, by order of the Emperor Paul, the Dépôt of Maps was instituted, which placed the geodetic work upon a firm foundation. Soon after Schubert began a course of lectures on geodesy with a view to training officers for operations of this character. He introduced into use the reflecting sextant, a pocket chronometer, and a simple portable telescope. The troublesome years at the beginning of this century put a stop to geodetic work.

After the war with Napoleon was over scientific work of all kinds was rapidly pushed forward, and the desire to have good maps caused many new ideas and instruments to be brought into use. Methods of reduction were introduced from France, while Munich furnished the theodolites and telescopes. It was apparent at this time that triangulation alone could furnish a basis for a good map.

In 1816 the Agricultural Society of Livland concluded to have a provincial map made, and asked Struve to assist by making a triangulation for them. In the mean time Tenner was covering Vilna with a network of triangles. It rested on three bases measured with an apparatus constructed on the Borda principle. Later the metallic thermometer was discarded, owing to the irregular behavior of the components, and Tenner devised an apparatus consisting of but

one bar with temperature obtained from two thermometers whose bulbs were inserted into the body of the bar. In this work the angles were measured by repeating circles made by Troughton.

Struve measured his base-lines with wooden bars, and his angles with a sextant, but these rude appliances yielded good results, at least in awakening in him the desire to undertake an arc-measurement. So enthusiastic was he that in 1820 he secured from the University of Dorpat the grant of a fund sufficient to carry the work along during the succeeding decade. Near the church Simonis, a base-line was measured with the apparatus invented by Struve which subsequently served as a pattern for all forms of apparatus used in Russia. It consists of one normal or standard bar and four measuring bars, each two toises long. One end of each bar has a contact lever abutting against the end of the following bar; on a special graduated arc is read the indication of the contact lever by which is computed the actual length of the bar at the time of measuring. Thermometers inserted in the bar showed temperature, while inclination was indicated by a level attached to the bars. The angles were read with a large universal instrument with four verniers. In this work Struve abandoned the method of

repetition, and began to measure the angles by shifting the limb according to the English plan.

After finishing this arc of about $4\frac{1}{2}°$, Struve determined to devote his energies towards its extension northward as well as southward. Along the Dorpat meridian there are no natural obstacles, the entire country in these directions being a plain altogether appropriate for degree-measurements. Appointed director of the observatory at Pulkova, founded in 1839, and being at the head of all astronomic and geodetic operations in Russia, Struve began to work still more energetically at his favorite scheme, which, owing to the protection of the Emperor Nicholas, he at last succeeded in carrying out; and before his death saw it finally discussed and published. This great undertaking was under way more than forty years, and embraced the various chains of Tenner and Woldstedt extending from Austria to Tornea. For the continuation of the arc farther north the coöperation of the Swedish government was necessary, as the stations were sometimes in Sweden, and finally in Norway. In 1844 Struve made a journey to Stockholm, received the consent of King Oscar, began the work at once with the assistance of Selander and Hansteen, and finished all in 1852, making an arc of 25° 20′. In this gigantic arc there are 258 triangles resting upon ten base-lines

placed at equal distances along the arc. Seven of these were measured with Struve's apparatus and three with Tenner's. Upon examining the junction lines, whose lengths were computed from two different bases, the average discrepancy will be found to be 0.1718 toise. The amplitudes were taken from astronomic observations for latitude at thirteen stations, or only one station for each two degrees. This would suffice for latitude, but it appears that azimuths were determined at these stations only.

The values for a degree were: 57,092 toises in latitude 53° 20′, 57,116 in 55° 34′, 57,121 in 56° 52′, 56,956 in 57° 28′, and 57,125 in 59° 14′. But while they are somewhat discordant one with another, still they have entered into all the recent determinations of the figure and dimensions of the earth, the mean length of a degree being taken, which is approximately for the parallel of Dorpat.

Two large arcs of parallel have been measured in Russia, in latitude 47½° and 52°. The motive of the southern arc was the proposal made in 1826 by the French government, that as there was an uninterrupted triangulation from Brest to Tchernowitz, it would be of great assistance in studying the figure of the earth to have this arc carried farther east. It was not until 1848, when the trigonometrical operations

reached southern Russia with the intention of covering the so-called New Russia with a network, that the chief, General Wrochenko, received instructions to perform his work so that amongst his triangles there should be an unbroken chain through all of southern Russia approximately along the forty-seventh or forty-eighth parallel; and to perform all operations with the greatest care, having continually in view the determination of a degree of longitude.

These instructions were implicitly obeyed during the following eight years, in which time the arc had been prolonged until it comprised 20° of longitude. In its entire extent there are three bases measured with the Struve apparatus. The determination of the amplitude of an arc of a parallel depends upon difference of longitudes of the terminal points. At the time when the triangulation was under way no very satisfactory method was in use for making such determinations, but since the telegraphic method has been perfected five stations along this arc have been occupied, but the results have not yet been published.

The other arc, known because of its extension as the central European arc of parallel, follows the fifty-second parallel from Haverfordwest in England, to Orsk on the river Ural, 39° 24′ being within Russian territory. This

parallel was selected because along it, especially in western Europe, there were already accurate triangulations. In 1860, when this work was definitely decided upon, there were in Russia only comparatively few triangles measured with sufficient care to be included in this important arc. It was therefore necessary to re-observe many of the old triangles and to add many new ones. Owing to various obstacles, especially while crossing the marshes of Minsk, covered with forests centuries old, where it was necessary to erect signals 150 feet and more in height, the entire work was not completed until 1872. Within the boundaries there are belonging to this chain 321 triangles, resting upon seven base-lines. Longitude determinations at fifteen stations were made mostly by Russian geodesists. The time determinations were made with a portable transit, the latitudes with a Repsold circle. For the transmission of time telegraphic signals were used, by noticing the turning aside of the needle of the galvanoscope. Each difference of longitude was determined six times, counting as a determination, two determinations of time between which electric signals were transmitted in four groups of twelve signals each. To secure independence of observations, the signals were given at unequal periods of time from thirteen to seventeen seconds. The

results of this work are not published yet, but taken from the manuscript it can be said that the difference between the observed and computed difference of longitudes varies from 5″.87 to 35″.52.

From the Russian work alone the earth's elements have been computed by General Bonsdorff, who gives for e 1 : 298.59. The standard used in Russia was a copy of the *toise du Pérou*, made by Fortin of Paris.

Although this practically concludes the account of the arc-measurement in Russia, it may be well to remark that the general topographic work begun under General Schubert in 1820 is still in progress under the general direction of the Military-Topographic Corps, of which Schubert was the first chief. The reports of this institution, printed in Russian, have reached the 47th volume.

Local attraction has received considerable attention from the Russian scientists, who found it difficult on any other hypothesis to account for station errors, which in some cases reached as much as 35″.76.

The pendulum also has been regarded as an instrument of considerable value in determining the ellipticity of the earth from the relative force of gravity at different stations. Sawitch, from the observations at 12 stations, obtained for e 1 : 309.

CHAPTER X.

SYSTEMATIC WORK IN SWEDEN AND NORWAY.

FROM what was said in a preceding chapter it might be supposed that Napoleon suggested a re-measurement of the Lapland arc. Melanderhielm, for many years president of the Royal Society of Sweden, when he heard of the labors of Delambre and Mechain, wrote to the former and obtained from him at the earliest date possible an accurate account of their work. His response was such as to confirm Melanderheilm in his belief that the polar arc needed a revision; consequently, when the king referred to his academy the question of its reliability, and whether French aid was necessary, it is quite natural that, while replying affirmatively to the first question, they should answer the second with a decided No!

Soon after this a grant for the work was made, and Svanberg was sent north in 1799 to reoccupy the stations of Maupertuis. He found a number of errors in the angles, and noticed that the river Tornea had a considerable fall where the base had been measured. These

considerations, supplemented by the fact that Maupertuis was ignorant of the exact coefficient of expansion, made it plain that it was of great importance to re-determine all of the elements of this arc. This was definitely decided upon and Svanberg, accompanied by Oefverbom, Holmquist, and Palander, started in the spring of 1801 for their field of labor. They were provided with a Borda repeating circle and copies of the standard metre and toise, which the National Institute of Paris presented, to show their interest in the work.

The base-line was laid out so as to, partially at least, coincide with the French line, but as the terminal monuments could not be found, the second measurement could in no respect be regarded as a verification of the former. The apparatus consisted of fine bars of wrought iron, each six metres in length. The terminal parts were lines drawn on the upper surface near each end, and contact was effected by bringing the two lines into coincidence. The only new feature introduced was the method of ascertaining the temperature. For this purpose there was a well sunk in the upper surface of the bar, and this partly filled with quicksilver. This quicksilver was supposed to have the temperature of the bar; hence, when the bulb of a thermometer was placed in this bath, they felt sure that

they had the temperature of the bar. The average temperature was about 8° F., while the temperature at which the bars were compared was at freezing point. In correcting for temperature, Roy's coefficient of expansion for wrought iron was employed. Inclination was ascertained by placing on the bar a level, one end of which, sweeping over a graduated arc, gave the angle of elevation or depression. The length of the line corrected for temperature and inclination gave 14,451.116 metres. The line was measured only once, so it is impossible to form an opinion as to its accuracy, but Svanberg merely states without going into details that it agreed with the base of verification measured later with wooden rods.

The signals were in the shape of a rectangular pyramid, provided with a centre post projecting above the supports and coming down sufficiently far to be braced to them. This centrepiece, however, did not come within two metres of the ground, so that when observing the instrument could be placed directly under the axis of the signal without moving the latter. This form of signal is still used in secondary triangles.

The original arc of Maupertuis was considerably extended, so that it had an amplitude of 1° 37′ 19″.57, and the value obtained for a degree was 57,209 toises. This has been cor-

rected by Airy, because in calculating the effect of nutation the moon's node had been used instead of the moon's mean place, giving for a degree in latitude 66° 20′ 12″, 57,198.9 toises.

The general excellence of this work is good, especially the terrestrial part of it. But the astronomic observations were not made with the care they deserved, and again, there is some doubt as to the temperature at which the standards were compared. In addition to this the temperature at which General Roy obtained the coefficient of expansion of iron was so much higher than Svanberg's mean temperature, that it is not certain that he used a reliable value for this coefficient, since the behavior of metal is not the same at all temperatures. Notwithstanding all this, this arc enters with more or less weight into all the discussions of the earth's figure.

This is practically the extent of Sweden's contribution to geodesy. Since the time of Svanberg the entire country has been covered with a most excellent network of triangles for topographic maps. Strictly speaking, they are not of a character to participate in a degree-measurement, but all of the trigonometric stations are permanently marked, so that whenever it is deemed advisable to reoccupy these stations to strengthen a chain of triangles it can be easily done. As Sweden is a member of the International Com-

mission for the Measurement of Degrees, this revision may at any time be expected.

While discussing this division of the subject, it is well to mention an invention of Jäderin, a Swedish professor, first announced in 1885. It is an apparatus for rapidly measuring a base-line, and consists of two wires, one brass and one steel, each twenty-five metres long. In measuring they are supported on tripods and are drawn taut by means of a spring balance. Two sliding scales on the head of the forward tripod are moved until a fixed mark on one coincides with the zero mark on the steel wire and the mark on the other scale coincides with the zero on the brass wire at the instant the spring balance registers 10 Kg. In moving forward the steel wire has its rear end set on its scale, and the brass wire on its. Thus it is seen that two independent measures are made, and the differences in the lengths of these wires are cumulative, finally becoming large enough to measure. Then it represents the total difference owing only to the relative expansions, and by knowing the coefficients of expansions it is easy to tell the mean temperature of the wires during measurement and consequently the exact length of the base at any desired temperature. The base-line at Moloskowizy was measured twice with this apparatus; the difference in the two results was

only one unit in 982,236. This same principle was announced independently by Mr. Eimbeck of the United States Coast and Geodetic Survey in 1878. It is seen that in reality this apparatus is simply a Borda metallic thermometer having a length equal to that of the base at a temperature equal to the average temperature of the measurement. It is interesting to notice in connection with base-measuring, how the forms of apparatus for a long time became more and more complex until they reached a climax in the Bäche-Würdemann, yet to be described, and then retracing the steps until it now seems as though the future bases would be measured with a single wire with repeated measurements and careful standarding.

Norway's early work in geodesy was limited to the aid given to Russia in the great arc already referred to. After that there was no apparent demand for a triangulation more accurate than would meet the needs of a good topographic map.

An association was formed in 1861, having for its object the general direction and supervision of such trigonometric work as could, when taken as a whole, be of value in arc-measurement. Most of the continental nations joined this association, and as one of the first objects outlined was to measure an arc from Palermo to

Levanger, or to the North Cape, it was necessary to have the coöperation of Norway. The delegates appointed on the part of this country soon found that the triangulation previously made would not answer for parts of this arc nor could the work be revised because of the obliteration in many places of all traces of the old signals. It was decided, therefore, to begin anew; consequently in 1864 two bases were measured with a slight modification of the Struve apparatus, one at Egeberg, near Christiania, and the other at Rindenleret, near the northern terminus of the arc.

In discussing the length of these bases, the commission in charge of the work exhausted the errors then known. They were: the uncertainty in reading the angle of inclination, error arising from the graduation of the arc over which the contact lever sweeps, error of alignment, and the error arising from the imperfection in the spherical surface in which the bars terminated at the end which impinged against the contact lever. There were likewise considered the errors due to the uncertainty in the accepted length of the rods. These were considered under four heads: (1) the error in the length of the standard rod; (2) the error due to the bending of the beam of the comparing apparatus; (3) the error in comparing the rods

with the standard; (4) the error due to the assumption that the diminution in length of the rods by abrasion was proportional to the length of time in use. After reducing them to the level of the sea, Egeberg base had a length of 2,025.28316 toises, with a probable error of 1:1,570,000, and Rindenleret base, 1,806.3177 toises, with a probable error of 1:1,500,000. Their toise was compared with Struve's, which had been most carefully compared with Bessel's.

The two base-lines which have been measured since then have had a somewhat smaller error. But as yet no junction has been made, consequently it is impossible to form an exact estimate of the accuracy of the work as a whole.

CHAPTER XI.

THE GEODETIC OPERATIONS IN INDIA.

THERE were a number of maps of the shore-line of India long before it was known how to make them with any degree of accuracy. But the credit of suggesting a systematic foundation for maps belongs to William Lambton, at the time a major in the Indian service. He also showed that at the same time valuable geodetic data would be accumulated, and nothing more needful than this existed, as the arcs in low latitudes were limited to those in Peru, and they were by no means accurate. This proposition was made at a most opportune time — just as it was decided to take steps towards exploring and collecting accurate information respecting the vast territory which had been thrown open to the English at the close of the Mysor campaign.

Although Lambton's project was approved at once by the Madras government, followed by his appointment as head of the undertaking, it was not until 1802 that instruments were furnished. These were a theodolite, zenith sector, and steel chains. The three-foot theodolite was captured

by the French frigate Piemontaise and landed at Mauritius; but it was eventually forwarded to its destination by the chivalrous French governor, with a complimentary letter to the governor of Madras. The zenith sector had a radius of five feet, and was made by Ramsden. The chain was historic, having been sent to the Emperor of China and refused. Another chain of 100 feet, made by Ramsden and compared at 50° F. with his standard bar, and a standard brass scale of three feet, completed the instrumental outfit.

The fixed point of departure was Madras, near which a base-line was measured between the 10th of April and the 22d of May, 1804, using the chain fitted into five coffers of wood, each 20 feet long, which were supported on tripods with elevating screws. The triangles were carried across the peninsula with a view to obtaining the length of a degree perpendicular to the meridian, and then northward. Although great care was taken to obtain the amplitude of the former from astronomic determinations of longitude, it was never introduced into the computations of the figure of the earth. It, however, reduced the width of the peninsula by forty miles, and thus showed the importance of an accurate trigonometric map.

This northward arc was to form the beginning

of his first "Great Arc Series," and was in 1806 extended south as far as Cape Comorin, where a base was measured. Two year later another line was measured, and this time the chain was laid upon the ground and drawn tight by means of two capstans. While passing over the flat country Major Lambton made use of the towers of the pagodas, on which scaffoldings were erected. In hoisting the three-foot theodolite to the summit of one of these pagodas, one of the guys gave way, and the instrument was dashed with great force against a wall, distorting the limb and damaging the tangent screw. He at once took the instrument apart, cut in a large flat plank a circle of the same size as the limb, and by means of screws, wedges, and pulleys he forced, after six weeks of effort, the limb into this circle. Its form was almost perfectly restored, and it was in constant use during the next twenty years.

In 1811 he turned all his force and energies towards carrying his arc as far north as the Himalayas. In commenting on the completion of this arc of 10 degrees he said: "In twenty years devoted to this work I have scarcely experienced a heavy hour. Such is the case when the human mind is employed in pursuits that call its powers into action. A man so engaged, his time passes on insensibly, and if his efforts are successful his

reward is great and a retrospect of his labors will afford him an endless gratification. If such should be my lot, I shall close my career with heartfelt satisfaction, and look back with unceasing delight on the years I have passed in India."

Lambton died in 1823, and was succeeded by Everest, who had been connected with the survey as assistant for five years. Everest presents a striking example of what will-power can accomplish. He was attacked with a severe fever, and his limbs were paralyzed. Still he persevered, believing that if he broke down the survey would disintegrate. He was lowered into and hoisted out of his seat by two men, when he observed with the zenith sector. But he finally felt obliged to go to England for his health, though he still held his appointment.

Colonel Everest was abroad from 1825 to 1830, but he was by no means idle. He spent his time inspecting instruments, and superintending the construction of those which he deemed best suited to his purpose, so that when he returned he was provided with a 36-inch theodolite, two double vertical circles three feet in diameter. But the most important improvement which he carried back was the compensating principle which Colby had applied to base apparatus. A set of bars constructed on this principle were at once put to use in the measurement of

the Calcutta base, about $6\frac{1}{2}$ miles in length. The extremities of this base were marked by two towers 75 feet high, to overtop the trees and houses.

As much of the country was covered with thick groves of mango, it became necessary to erect signals high enough to raise the instrument above them. The signals, in order to possess the requisite stability, were built of masonry. They were square at the base, about fifty feet high, with walls five feet thick at the bottom and two at the top. The roof or terrace was supported by two large stone beams on which rested a cylindrical well of masonry, surmounted by a circular slab of stone. At right angles to these stone beams and $3\frac{1}{2}$ feet above them, were rafters supporting the stage for the observer, round which was a hand-rail to which the observing tent was made fast. Thus the instrument was completely isolated from the stage on which the observer stood. The theodolite was hoisted up by a crane at one corner. It was while engaged on this work that Everest, fearing that the great theodolite had been injured by its fall, decided to systematically employ all parts of the circle, giving rise to the method now in use: that is, to make 5, 7, 11, 13, 19, or 23 series, by dividing the circle into such a number of parts; as each of these numbers is prime to two or three reading microscopes, no microscope can fall on

the same part of the limb twice in measuring the same angle. Suppose it is decided to make eleven series, the initial pointing for each set of the series is first made. One eleventh of $360° = 32° 43' 38''.2$; two elevenths $= 65° 27' 16''.4$, etc. The first set is made by starting from zero, bisecting each signal in order, and returning; then reverse the telescope and repeat the pointings; this constitutes a set. Then the instrument is set approximately at $32° 43' 38''$, pointed to the first signal, then to each other one and afterward reversing. When all of the initial settings have been exhausted, a series is completed. At each station as many series are observed as may be necessary. In this work Everest used the heliotrope, a circular mirror fitted for vertical and horizontal motion, and so directed at one station that the sun's rays are reflected towards the station occupied. This instrument was invented by Gauss in 1820, and was at once generally adopted.

In 1841 there occurred for the first time a good opportunity to test the accuracy of their work. In this year the Bidar base was measured with the compensating apparatus, and agreed within 4.296 inches with the length as computed from the Sironj base 430 miles distant. This closed the work on the great arc which extended from Cape Comorin to Boang in the Himalayas. In these observations Ever-

est used triangles that were as nearly isosceles as the country permitted, allowing no angle less than 30° and none greater than 90°.

Sir A. Waugh, formerly an assistant on the Survey, became its chief upon the retirement of Everest, and carried forward the work with the enthusiastic zeal and attention to detail which characterized his predecessor. He especially extended what is called the gridiron system of triangulation, that is, to have approximately parallel meridional chains of triangles, intersected at intervals by longitudinal chains. These serve as the bases of the secondary triangulation which fill in the interstices and in turn serve for plane-table points. These chains have also furnished data which superseded the old results that entered the problem of the figure of the earth.

Every possible care has been exercised to secure results free from the effect of local attraction, since the great mass of the Himalayas and the depth of the Indian Ocean have suggested that the deflection must be present to a marked degree. This has stimulated the investigation of this disturbance, and good results have been the outcome.

As completed, the primary triangulation has an aggregate length of 17,300 miles, contains 9,230 first-class angles, all observed, rests on

eleven bases measured with the Colby compensating bars, and is fixed in position by latitudes observed at 84 stations. To have taken this as a whole, and subjected it to the conditions already mentioned, would have required the solution of equations involving 9,230 unknown quantities. As this was practically impossible, the bulk of the triangulation was divided into five sections, each of which was treated in succession with as close approximation to the mathematically rigorous method as was practically possible; but even then the number of quantities introduced gave unwieldy equations to solve, exceeding anything of the kind ever attempted. The result, however, was gratifying, leaving residuals even less than the anticipated probable error.

The longitudinal arcs are by no means insignificant, and are gradually contributing geodetic data. This, however, can only be done as the telegraph lines are extended so that differential longitude may be determined thereby.

The aggregate length of the meridional arcs already completed in India is about equal to that of the English, French, and Russian arcs combined, but the longest individual arc in India is somewhat shorter than the Russian.

The Indian survey has made valuable contributions to geodesy in an extensive series of pendulum observations for determining varia-

tions of gravity. This instrument, which now promises so much, throws light on the relative lengths of the terrestrial axes and hence the ellipticity. Observations with the pendulum were commenced in 1865 on the recommendation of the Royal Society. One of the pendulums was the one which Sabine had used in his long series of operations. Two others were afterwards added; and all three have been swung at many stations in India, at the Kew Observatory, and were brought to America by Colonel Herschel. The pendulum operations in India have been successful in removing the reproach lately cast upon the geodetic work, that its value has become greatly diminished by the discovery that the attraction of the Himalayas is much greater than had previously been suspected, that it must have deflected the plumb-line considerably, and consequently injured the arcs. But the Indian arcs alone give a value for the ellipticity so near to the value obtained from the arcs measured in all parts of the globe that it is evident that the deflection of the plumb-line has not seriously affected the amplitudes.

CHAPTER XII.

GEODETIC WORK IN PRUSSIA.

Geodesy in the German States has had a significance somewhat different from what it has in this country. There, it includes operations which here are covered by the term surveying, so that in this chapter discussion will be limited to those trigonometric operations which have been made with a view to aid in the determination of the length of degrees. This class of work began in 1802 with the measurement of a base-line near Seeburg by Von Zach, assisted by Von Müffling. The line was carefully measured and the termini marked by cannons given for that purpose by the Duke of Weimar. They were carefully imbedded in masonry, with the open end up. In the mouth a cylinder of brass was fastened by having lead run around it, and above the whole a stone pyramid was built. The triangulation began in 1805, but was stopped in 1806 on account of the war with France, although Gotha, the province in which the work was being prosecuted, remained neutral. After the battle of Jena, the people of

Gotha, fearing that the French would not regard their neutrality self-imposed or lasting, especially if they should be suspected of harboring concealed weapons, caused the cannons to be dug out and destroyed, thus sacrificing some accurate work to allay a fleeting fear. Lindenau subsequently tried to find the terminal points of this base, but could not locate them with certainty.

When Von Müffling became quartermaster-general he was in a position to resume the work which he had laid aside, and proposed to measure an arc of parallel extending from Seeburg to Dunkirk. He proposed to determine the difference in longitude by powder flashes, that is, to note in local time the instant the flash was made at one point, and at the other station the instant it was seen, also in local time. The difference of time would be one fifteenth of the difference of longitude. In the spring of 1817 he laid his proposition before the Bureau of Longitudes in Paris. It was referred to a committee to consider which would be better to use, powder flashes or flashes of light from a reflector. No report was ever made, and when the German army was withdrawn from France the project fell through. He, however, took the triangles already observed, and projecting the suitable lines he was able to compute the large

triangle Seeburg, Dunkirk, Mannheim. He found the difference of longitude of the two stations first named, and deduced for the ellipticity 1:315.2. About this time he proposed a continuation of this chain to Memel, a station in the Russian net, but when he left the army soon after, the scheme was forgotten.

Gauss began in 1820 a triangulation to connect the observations of Göttingen and Altona. His work rested upon the base-line of Schumacher, measured that year in Holstein. The angles were read with a twelve-inch theodolite, using the method of directions. This was the first time that the method of repetition was abandoned on the Continent, and the good results obtained, principally owing to other improvements, caused many to look with disfavor on Borda's scheme. This work also witnessed the first use of the heliotrope, invented by Gauss. In its first form it was somewhat complicated; the one now used has been improved by Bessel and others. Another of Gauss's discoveries found application in this work, that is, the method of least squares. These three improvements are perhaps the greatest which this century has witnessed in geodesy, and they were made while extending an arc of only 2° 0′ 57″ amplitude. The resulting length for a degree, 57,126 toises in latitude 52° 2′, has received

especial confidence, partly because of the care with which the amplitude was determined; the termini being fixed observatories, observations could be made at leisure and under the most favorable conditions. Gauss realized this great advantage, and earnestly expressed the hope that all of the observatories of Europe might be united by triangulation.

About this time many of the states began an independent triangulation, or joined some of the nets already reaching out in various directions, but they were chiefly for cartographic purposes. However, there is one isolated undertaking of importance which does not belong to this class, that is, the work of Schwerd.

Schwerd realizing that the base-lines, which at his time were from six to eight miles in length, were the weakest part of the trigonometric work, set about in 1820 to see how improvements in this direction might be made. These long bases required the labor of several men many weeks for a single measure, and demanded for accuracy that the apparatus should remain during this time constant; besides this, it was difficult to find a locality suited for a line of this length. He concluded that a short line which could be measured several times under more favorable conditions would serve as a base for a longer line to which it could be joined by the strongest

possible triangulation, this longer line becoming what he called a trigonometric base. With these theories in mind Schwerd measured the little Speyer base twice, having a difference of 0.000895 m. in 859,441 m. or an error of one unit in one million.

In discussing the results he investigated the effect of twelve sources of error — one, the errors of graduation of the thermometer which gave the temperature of the bar, was here used for the first time. The trigonometric base of nearly 5,000 metres was computed by three different routes, giving a probable error of .0054 m.

Schwerd used a bar of one metal: this and his short base were steps in the right direction, though taken long before any one was found ready to follow therein. Here again we see the reactionary movement from the long bases and complicated forms of apparatus, which is now making itself felt again.

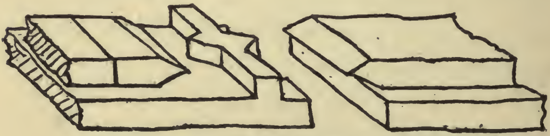
The work for whose completion the nations of the world will be ever grateful to Prussia is the degree-measurement executed in East Prussia by Bessel and Baeyer. In 1829 the Russian government, having extended their triangulation to their southwestern border, requested the Prussian king to empower the director of the Königsberg observatory to connect their net with his observatory. Gauss had shown how beneficial it was

to tie to a station whose geographical position was fixed beyond a question of doubt. Besides this, there were already to the westward arcs in Hanover, France, and England, with only a gap in Prussia to be filled to unite them with the arc in Russia.

It apparently required but little persuasion, for the king soon issued the necessary orders, and Bessel at once prepared for the undertaking. In theory he was perhaps the best equipped man of his time, but was deficient in practical geodesy. Fortunately for him, and for us too, there had been introduced to him only a few years before this, Baeyer — a young lieutenant — who had publicly received the thanks of Von Müffling for his valuable aid in making his triangulation.

In the operations which began in 1831 it is impossible to separate the work of these two colaborers, Bessel and Baeyer; so harmoniously did they carry forward their undertaking that, at its close, neither could point to any integral part and say "this I did." It was decided that they should not limit their task to the simple trigonometric connection, but that they by measuring a new base would have a net independent of the work of others. This was well, as there was no test of the accuracy of the parts finished by them. The base apparatus was devised by Bessel; at least it bears his name. It was constructed on the

Borda principle, the metals used being iron and zinc, and the changing length was measured with a glass-wedge. The iron bar was the longer, and supported the zinc on its upper surface. The zinc bar terminated at its free end in a horizontal knife-edge, and the iron bar very near and beyond this had attached to itself a piece of iron with a vertical knife-edge on both sides in the direction of the length of the bar. The distance between the end of the zinc and this fixed point, changing with the varying temperature, was measured by means of a glass wedge, whose thickness varied from 0.07 of an inch to 0.17 of an inch, with 120 divisions engraved on its face, the distance be-between the lines being 0.03 of an inch. The other vertical knife-edge, projecting slightly beyond the end of the bar, was brought in measuring very near the horizontal knife-edge,



Bessel's Base Apparatus.

in which the opposite end of the other bar terminated, and the intervening distance measured with the same glass wedge. If the wedge in this case be carefully read, and its thickness at each division accurately known, this method

eliminates some of the uncertainties inseparably connected with the method of contact. The illustration here given shows the arrangement of the knife-edges in the two ends. With this apparatus they measured in 1834 the base-line at Trenk, going over it twice with a difference of one sixth of an inch in 934.99 toises.

The ends of the base were marked by a pier of masonry inclosing a granite block, in whose top was set a brass cylinder carrying cross-lines on its upper end, the intersection of which marked the end. Just above this was built a hollow brick column high enough for the theodolite support, with a large square stone for a cap-stone. In the centre of this there was a cylinder co-axial with one below, so that the instrument while the angles were read could be placed directly over the termini of the base.

The theodolites had 12 and 15-inch circles, read by two verniers using the method of directions. In the matter of signals two forms were employed. One consisted of a hemisphere of copper placed with its axis vertically over the centre of the station. The sun shining on this gave to the observer a bright point, but as this line was not in a line joining the centres of the stations observing and observed upon, a correction depending on the relative positions of the sun and the observer had to be applied. The

other form consisted of a board about two feet square, painted white with a black vertical stripe ten inches wide down the centre. This board was attached at a suitable elevation to an axis made to coincide with the centre of the station, so as to permit the board to be turned until it was perpendicular to the line of sights. As different stations were occupied the corresponding changes were made in the direction of the board at each observed station.

Astronomic observations were made with extreme care at three stations. And as the chain was oblique, by knowing the latitudes of the terminal points, and the direction of the line joining them, it was possible to determine from this arc the length of a degree of a meridian and of a degree perpendicular thereto. The stations were so situated that this direction was obtained from the azimuth of three lines only. The amplitude of the arc of the meridian was $1° 30' 28''.98$, using the two parts into which the arc was divided by Königsberg, the difference in the distance between the terminal points taken as a whole and the sum of the two parts was only 0.973 toise, which is an evidence of the great accuracy attained in this work.

Up to this time the method of least squares had been applied in triangulation to ascertain the most probable value of those angles only

which were required in the computation. But Bessel measured at each station a number of superfluous or auxiliary angles, and they too were introduced to aid in obtaining the best values for the essential angles. This involved seventy quantities, but the labor of solution was compensated for by the gratifying results.

Bessel was not associated with any active geodetic work after this, but it was only the beginning of Baeyer's career. In 1843 he was placed in charge of the trigonometric branch of the War Department, completing in this capacity the coast survey, and pushing forward the interior surveys. In 1861 he organized the Central European Degree-measurement Commission, and seven years later the Geodetic Institute of Prussia. The latter has had charge of all matters pertaining to geodesy from that time until 1885, when Helmert, Baeyer's successor, turned over the trigonometric work to the Land Survey, and retained that branch of the work which pertains to astronomy, local deflection, refraction, and gravity determinations. At the present time there is no other institution so well equipped for investigation along those lines, nor can any boast of a more accomplished chief.

There only remains, in this connection, to speak of the base apparatus recently constructed for the Institute. It consists of platinum and iridium bars on the Borda principle, resting on

an iron truss, with the entire length of the bars open to the free circulation of the air. Inclination is determined by a level of precision, and flexure by a movable level.

There are six microscope stands, the same number of trestles for the bars, and thirty sets of heavy iron foot-plates. The latter are put in position, and remain there half a day before being used. For each microscope stand there are two telescopes — one for aligning and one for reading the scales. For manipulating this apparatus six skilled observers and thirty laborers are needed.

Closely connected with the work in Prussia are the operations in Italy, beginning in 1815, Austria, 1839, Belgium, 1844. Spain made its beginning in 1858, following the general plan adopted in India. In all of these countries the triangulation was made primarily for cartographic purposes, but upon the organization of the European Degree-measurement Commission each obtained therefrom instructions as to how the work might be revised or extended so as to make it available for geodetic data.

Belgium has shown us how to measure a line in two sections which make an angle with one another, Austria is adding to our pendulum data, and Spain has given us a wonderful example of what can be done in the way of exact measurement of base-lines.

CHAPTER XIII.

GEODETIC WORK IN THE UNITED STATES.

As the ideas of a people as well as those of an individual are strengthened and improved by contact and association with its fellows, so each nation has advanced its interests by intercommunication, or, failing in this, has dwarfed them by confinement. Hence one might take as the exponent of a nation's prosperity the amount of wealth that is going to and fro on its high seas. And though for many years after the arrival of the first settlers in America the attention of her people was feverishly directed towards the discovery and development of her resources, still the dependence upon the parent lands for those articles which the new enterprises failed to supply, the alluring call for recruits and their prompt responses, were such as to stimulate a maritime intercourse that has grown with each passing year.

The shores which skirt our most frequented ports have been again and again strewn with wrecks, each bearing sad testimony to our igno-

rance regarding the currents, shoals, and reefs, while hundreds and thousands of lives have been the *price* of that same ignorance. To supply just such information, every enlightened nation having a foreign commerce at stake has instituted an accurate survey and delineation of its entire ocean boundary, not only as to its apparent outline, but also as to the character of the soundings leading into deep water. The successful prosecution of such operations necessitates enormous outlays, but the assuring thought swiftly comes that the loss of one vessel more than equals the amount which a century of work would have cost around the spot where she sank.

The first appeal to our general government for assistance in carrying on a survey of the coast came from Messrs. Parker, Hopkins, and Meers, who in the latter part of the last century made the necessary observations and examinations and collected all the data requisite for a chart of the coast of Georgia from St. Mary's to Savannah, together with its harbors, rivers, and inland navigation. In doing this they exhausted their resources, and to enable them to have their chart engraved they petitioned Congress to make an appropriation of $3,000. This petition was referred to a committee, which in its report, submitted February 27, 1795, expressed an opinion

favoring the work in general, but without an acceptance of this special proposition. The conclusion of the report was a series of resolutions, one being: "That the President of the United States be requested to obtain as soon as possible complete and accurate charts, made from actual survey and observation, of the seacoast, from the river St. Mary's in Georgia to Chesapeake Bay, inclusive, and that —— dollars be appropriated for that purpose." Another provided that the work be done state by state, that results already collected might be purchased, that revenue cutters be employed, etc. These suggestions were referred to another committee, which, on December 29th of the same year, made a report almost identical with the first as to preamble and the first resolution just quoted, but containing no recommendation as to the ways and means for prosecuting the survey.

These memorialists, either because of their interest in the work, or because of their desire to be reimbursed for the outlay they had already made, persevered until they secured a consideration of their petition by the Committee on Commerce and Manufactures. This committee on May 14, 1796, recommended "That the President of the United States be requested to procure such accurate charts of the Atlantic coast of the United States, including the bays, sounds,

harbors, and inlets thereof, as have been made from actual observation and survey; and that, in all those parts of which no actual surveys have been made, or where the same shall, in his opinion, be inaccurately done, he be requested to employ proper persons to survey and lay down the same, and to order the revenue cutters of the United States on that service, whenever, in his opinion, it can, without inconvenience to the public, be done." From this it appears that the President was to be the first superintendent of the survey. There was also another resolution providing that whenever the work of any person or persons was accepted, compensation be tendered, together with the right for the maker to publish the charts and copyright the same. Although this method of prosecuting the survey of the coast was never put into practice, nor were Messrs. Parker, Hopkins, and Meers ever compensated for their work, this petition was unquestionably the first public expression of the needs for a coast survey.

The next step taken in this direction was the outcome of a suggestion of the Committee on Commerce and Manufactures in its report, made February 27, 1806, upon the expediency of making a survey of the shoal of Cape Hatteras, Cape Lookout, and the Frying Pan. The significant clause was: "The committee are

fully apprised of the importance of having accurate surveys of the whole American coast, and they ardently hope that Congress will, at the next session, direct a complete examination to be made of it, from the St. Croix to the Mississippi, and to the extreme southwestern part of Louisiana, in the Gulf of Mexico. . . . Such a survey is much wanted, and we ought not any longer to rely on foreign charts for a knowledge of our own coast, when errors and omissions of great magnitude are known to exist in by far the greater part of them; and when, too, it is considered that corrections are seldom made in the American copies, it is presumed that there can be no doubt of the propriety of directing the earliest attention to this interesting subject." This report also embodied provisions for completing a survey of the coast of North Carolina. Two commissioners, Thomas Coles and Jonathan Price, were appointed to perform this work, which they completed during the following summer. In submitting their report to Congress, Mr. Gallatin, then Secretary of the Treasury, spoke of their chart as being more correct than any extant.

This amount of space has been given to the antecedents of the coast survey, in order that it may be clearly understood to what extent it was indebted to them. Just who it was that sug-

gested a map resting on a triangulation is not known, but it is the general impression that it was Robert Patterson, at that time director of the mint at Philadelphia. Being acquainted with President Jefferson and the members of his Cabinet, he had free access to them; this, added to Jefferson's enthusiastic interest in all scientific matters, made it an easy task to secure at least a consideration of any project that he might suggest. At all events, the act of Congress of February 10, 1807, was passed upon the recommendation of the Executive. It appropriated $50,000 "for making complete charts of our coast, with the adjacent shoals and soundings."

The best means of putting this act into effect was not at once apparent to the President, nor to his Secretary of the Treasury, Mr. Gallatin; so the latter, under date of March 25, 1807, issued a circular setting forth a project for a survey, and requesting outlines of such a plan as might unite correctness and practicability. According to the project sent, the operations were to be under three distinct heads: 1. "The ascertainment by a series of astronomical observations of the true position of a few remarkable points on the coast; and some of the lighthouses placed on the principal capes, or at the entrance of the principal harbors, appear to be the most eligible places for that purpose, as being objects

particularly interesting to navigators, visible at a great distance, and generally erected on the spots on which similar buildings will be continued so long as navigation exists." 2. "A trigonometrical survey of the coast between those points of which the position shall have been astronomically ascertained: in the execution of which survey, the position of every distinguishable permanent object should be carefully designated, and temporary beacons be erected, at proper distances, on those parts of the coast on which such objects are rarely found." 3. "A nautical survey of the shoals and soundings off the coast, of which the trigonometrical survey of the coast itself and the ascertained position of the lighthouses and other distinguishable objects would be the bases; and which would, therefore, depend but little on any astronomical observations made on board the vessels employed on that part of the work."

The circular also asked for the names of such persons as could be recommended as capable of assisting in the different parts of the work. They were sent to Robert Patterson, Andrew Ellicott, Secretary of the Land Office of Pennsylvania, F. R. Hassler, then in Philadelphia, Isaac Briggs of Maryland, James Madison, President of William and Mary's College, and Joshua Moore of the Treasury Department. The replies of these gentlemen give the best insight attain-

able into the condition of applied mathematics of that time.

As a rule they advocated the determination of longitudes by finding local time from equal altitude observations upon the sun, and comparing this time with a chronometer set to the time of some known meridian, after allowing for rate. It was thought that this method would give results correct within two seconds of time. Latitudes were to be determined from meridian altitude observations on the pole star with an error not exceeding 10″ or 15″. The instrument should be a sextant or a Borda circle.

It was thought that 30 points so determined would be enough for the entire coast, with perhaps the position of a few intermediate stations determined by rockets, powder flashes, or balloons. The triangulation was a feature that taxed their ingenuity; because they appreciated the difficulties that would attend such a work over a low wooded country such as then prevailed along the coast. The accuracy they thought attainable permitted an error of 30″ in each angle, which, said one, "allowing for the errors in determining the position of the initial point, and supposing the base-line to be correct, would, in a distance of 1,000 miles, about the length of our coast, give an error of only 660 feet."

The instruments necessary for the work were

estimated to cost three or four thousand dollars; with, in the schedule of one, an item of fifty dollars for "an apparatus for measuring base-lines."

While the above is a digest of the plans generally submitted, there was a most notable exception in the response of Mr. Hassler. He came to this country in 1805, fresh from geodetic work, having been engaged in the triangulation of the Swiss Canton of Berne, and later enjoyed the rare privilege of working with Bohnenberger in Austria. He brought with him a rich experience, a valuable collection of instruments, and a large library of scientific works. This, added to a desire to lend assistance to every worthy undertaking in his line, placed him in a situation to make suggestions that were of great value in the organization of such an important work. He suggested that there be measured upon the whole extent of the coast, with a "cercle repetétur à deux lunettes," of one foot diameter, or with an English theodolite of at least the same diameter, and capable of multiplying angles, a chain of triangles, the sides of which should be about 60,000 or 100,000 feet, and established upon bases measured with the greatest possible accuracy.

All the astronomical observations and deductions which circumstances may require, or which may be necessary, ought to be made in the course of the work at convenient points, as well for the

determining the latitude and longitude of those points, as the azimuths of the sides of the triangles. At the same time, as many secondary points, and even simple directions, ought to be ascertained as can be effected without impeding the principal design, in this way fixing the situations of lighthouses, towns, villages, and other important points on the coast which would serve for the continuation of the surveys in detail.

The results should be laid down, according to the differences of the meridians and parallels, upon large paper, divided into sections for convenience, and accompanied with a table of longitudes, latitudes, distances, and azimuths.

The ways of keeping the records, taking soundings, and making the astronomical observations were discussed quite fully. He proposed to use for a signal a triangular pyramid from ten to thirty feet high, from which a pole should proceed, bearing a ball one foot in diameter, composed of potter's clay and covered with a yellow varnish, forming a good reflecting surface; or a cylinder made of barrel hoops covered with cloth. For night signals, large argand lamps with wicks six inches in diameter were proposed.

The sheets referred to above should be given to those who are to have charge of the detail survey, who should take the given points as bases,

from which to fill up their portions of the survey, as fully as may be desired, either with a small theodolite, the "planchette," a kind of plane table, sextant, or compass, according to the accuracy and amount of detail necessary.

For off-shore work he considered the three-point method unsafe, and advised the employment of two observers, one on land at a known station and the other on the boat, both of whom at the time of sounding should read the angle between the other and a second visible point.

The various plans were referred to a commission consisting of Messrs. Patterson, Briggs, and Hassler, who, after due consideration, agreed that they should recommend the scheme of Mr. Hassler to the President.

The unsettled condition of the political affairs of the country about this time prevented any further consideration of the survey project until March, 1811, when Mr. Gallatin requested a friend to ascertain if Mr. Hassler would undertake the mission to London for the purchase of instruments. An affirmative answer was at once given, and preparations for the trip begun, in the way of making drawings, and consulting with those in authority regarding details. Troughton, who was selected to make the instruments, was at the time working for the Greenwich observatory; this, together with a tedious

trip to Paris, and the troubles arising from the Rebellion, caused such serious delays that it was not until September 14, 1815, that the mission was ended by the delivery of the instruments to Mr. Patterson.

The plan for putting into operation the survey of the coast was submitted to Mr. Dallas, then Secretary of the Treasury, January 5, 1816, and the approval of the President was received a couple of weeks later. But it was not until May that Congress made the necessary appropriation, and Mr. Hassler's commission was not signed until August 3d of that year.

Even before his appointment as superintendent, Hassler spent several weeks seeking a place suitable for a base-line, visiting for this purpose portions of the New Jersey and Long Island coasts, finally finding a site in the Tinively Valley at the west foot of the North River Mountains. The difficulties experienced from wooded marshes, and lack of sharp points near the coast, impressed upon him more and more the necessity of carrying along back from the coast a strong chain of triangles, with secondary triangles to form the immediate basis of the off and in-shore work.

As had been expected, the instruments were in great need of adjustment, after a space of five years, in which time they had made a long

voyage, confined in boxes with unequal strain on their different parts. The small theodolites fared better than the larger ones. Still, Hassler was satisfied with the results, saying that his triangles closed within one second.

A second base was measured in December, 1817, upon the seashore of Long Island, near the Narrows. The resulting lengths of this line from three different combinations of triangles carried out upon it, falling within two tenths of a metre, were taken as confirmatory of the accuracy of the original base.

As was quite natural, the beginning of a work so vast, in a country almost in its natural state, without trained assistants and with instruments needing trial and adjustment, was a slow and tedious process; so much so, in fact, that the Secretary of the Treasury wrote to the Superintendent: "For it must not be dissembled, that the little progress hitherto made in the work has caused general dissatisfaction in Congress, which, if not removed, may lead to a repeal of the law under which you are now acting." In answer to this letter, Hassler gave a detailed account of his operations up to that time, showing a large amount of work completed in four months of actual labor. Perhaps if one could really know, it might be shown that others were instrumental in having an act of Congress

passed repealing all former acts relating to the appointment of a civilian superintendent, and placing the work under the direction of the War Department. Many engineer and other officers were left without duty by the peace of 1815, and the law of 1818, limiting appointments to the army and navy, looks as if they were seeking employment.

From this time until 1832 the survey of the coast was spasmodic and disconnected. In the latter year the law of 1807 was reënacted and Hassler again made superintendent. The work began at once in earnest and was carried forward with all needful care, but in 1842 there was a cry of extravagance which was so loud that a committee of Congress was appointed to investigate the methods in use. The scrutiny to which the work was subjected was of an unfriendly character, and the examination was addressed to the superintendent rather than to the work. The outcome was a complete vindication, but to satisfy all parties a board of reorganization was appointed. This board adopted a plan so much like the one already in use that it practically set its approval on Hassler's methods and results.

By way of apology for having devoted so much space to the early history of the Coast Survey, it can be said that the reports of all

operations subsequent to this period are accessible to the general reader, and that while contemplating the great work now in progress under its auspices, it will be profitable to have in mind the tribulations through which it passed.

Strictly speaking the Survey has made as yet no special contribution of geodetic data, but there is no branch of geodesy that has not been advanced by it, and at present the entire force is working on the very borders of that science. As has already been stated, the primary purpose of this institution was to make a survey of the coast; for this purpose numerous points only a few miles apart were needed. If they are located trigonometrically it gives small triangles in which errors increase with their number. Therefore in the very early history of the Survey it was deemed best to extend along back from the coast a chain of strong triangles to form the backbone of the smaller chains which at frequent intervals extended outward to check the coast chain. This primary chain soon attained such proportions that it was evident that it could be used for determining the length of a degree, and the elements of the earth. About this time it was suggested that in order to feel sure that the points on the Pacific coast were rightly located with respect to those on the Atlantic it would be advisable to connect the

chains on each coast by a transcontinental chain.

This was further emphasized in 1878 by the change of name to the Coast and Geodetic Survey. This parallel chain is not yet completed, and persons in many parts of the world are anxiously awaiting the results, for it will be remembered that the Western Hemisphere contributes only the untrustworthy Peruvian arc in the great problem of the figure of the earth. Surely this condition cannot remain much longer.

To sketch even the methods employed in the Survey would be to write in detail an elaborate treatise on geodesy. The general progress has been along lines followed by the geodetic operations of other countries — from the complex to the simple, and whether complex or simple methods were employed, no institution has done its work better; and with Superintendent Mendenhall at its head we can safely expect to see our Survey soon occupy its destined place in the forefront of geodetic institutions.

CHAPTER XIV.

CONCLUSION.

As the main object of geodetic operations is to determine the size and shape of the earth, it will be well to sum up the whole matter in a few final words. Regarding the earth as a spheroid, the following table shows the principal results that have been attained: —

DATE.	AUTHORITY.	ELLIPTICITY.	QUADRANT IN METRES.
1806	Delambre	1 : 334	10,000,000
1819	Walbeck	1 : 302.8	10,000,268
1830	Schmidt	1 : 297.5	10,000,075
1830	Airy	1 : 299.3	10,000,976
1841	Bessel	1 : 299.2	10,000,856
1856	Clarke	1 : 298.1	10,001,515
1863	Pratt	1 : 295.3	10,001,924
1866	Clarke	1 : 295	10,001,887
1868	Fischer	1 : 289	10,001,714
1872	Listing	1 : 286.5	10,000,218
1878	Jordan	1 : 286.5	10,000,681
1880	Clarke	1 : 293.5	10,001,869
1891	Harkness	1 : 300.2	10,001,816

The values which are the most frequently quoted, and those which have influenced geodetic tables and taken their places amongst the

solar constants, are those of Bessel and Clarke. The former obtained from ten meridian arcs, one each in Russia, Prussia, Denmark, Hanover, England, Peru, Lapland, France, and two in India, furnish those elements on which the "Bessel spheroid" is constructed. The "Clarke spheroid" rests on elements obtained from the Anglo-French arc, Russian, Cape of Good Hope, Indian, and Peruvian, comprising nearly eighty degrees of amplitude and including fifty-six latitude stations. This spheroid has only recently found general acceptance, and has entered only comparatively few geodetic tables.

There is every reason to believe at the present writing that the "Harkness spheroid," here for the first time mentioned in print, will very soon supplant all others. Professor Harkness, United States Navy, has recomputed all of the solar constants, imposing, while forming his equations, the condition that the final results shall be consistent one with another. One of the constants was the ellipticity of the earth, and in obtaining it, he allowed the best data from geodetic operations, the pendulum, precession of the equinoxes, and the action of the tides to have their proper weights. The resulting ellipticity can enter all of these observed phenomena with the minimum of incongruities.

It is a source of great pleasure and patriotic pride that the last page of this historic sketch can bear this testimony to the achievements of an American Mathematician.

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